JOURNAL
of the
AMERICAN CONCRETE INSTITUTE

PRINTED IN TWO PARTS—PART TWO

Vol. 27 | December 1955 | No. 4

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for

Vol. 51

ACI *Proceedings*, 1954-55

(JOURNAL Vol. 26)

to provide a comradeship in finding the best ways to do concrete work of all kinds and in spreading that knowledge

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Every year the Institute receives many requests for the Title Page and Index of the annual *Proceedings* volume long after inquirers have received them; also many inquiries about binding annual sets of JOURNALS and buying bound volumes from the Institute. Within the covers of this Part 2 of the December JOURNAL are Title Page, Table of Contents, Concluding Discussion, and Indexes for the volume otherwise completed with the June 1955 JOURNAL. On the back cover is an announcement about the availability of bound volumes from the Institute and about "gathering" the contents of a year's JOURNALS, plus this Part 2, for binding a *Proceedings* volume. Please see announcement on the back cover.

PROCEEDINGS
AMERICAN CONCRETE INSTITUTE

Volume 51

1954—1955

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from JOURNAL OF THE AMERICAN CONCRETE INSTITUTE (Vol. 26)

September to December 1954, January to June 1955
(10 JOURNAL issues) and
Part 2 of December 1955

PUBLISHED BY THE AMERICAN CONCRETE INSTITUTE
18263 W. McNichols Rd.
DETROIT 19, MICHIGAN

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Printed in U. S. A.

ERRATA and ADDENDA

The following correction should be made in "Admixtures for Concrete," ACI JOURNAL, October, 1954.

p. 126—In the first sentence under "Pozzolanic Materials" ASTM Designation C 129 should be changed to C 219.

* * * *

The following additions should be made in "Fundamental Factors in the Drying Shrinkage of Concrete Block," ACI JOURNAL, November, 1954.

p. 244—in the caption to Fig. 8 after the word "autoclaved" add "(solid line)" and after "normally cured" add "(dashed line)."

p. 246—in the caption to Fig. 9 add "Autoclaved block are represented by solid line; normally cured block are represented by dashed line."

* * * *

The following corrections should be made in "Ultimate Load Theory and Tests of Cylindrical Long Shell Roofs," ACI JOURNAL, November, 1954.

p. 260—the reference in the eighth line under "Inelastic buckling" should be changed from 4 to 5.

p. 261—in the definition of $\left(\frac{r}{t}\right)_e$ under "Notation," the phrase "(see reference 4)" should be changed to "(see reference 5)."

p. 262—in Eq. (4) S should be changed to s.

p. 262—in Eq. (8), (9), (10), and the line "V'_{max} = see Eq. (9)," V'_{max} should be changed to v'_{max}.

p. 266—in the last line "Eq. (1)" should be changed to "Eq. (2)."

p. 269—in the first and second sentences under "Example," reference 8 should be changed to reference 9.

p. 269—the line under "Dimensions" reading "β_T = ... 0.64577 radians" should be changed to "0.6458 radians."

p. 269—in the fourth line under *Tension flange steel*, "β_{NA} = ... 0.19373 radians" should be changed to "0.1937 radians."

p. 269—in the last line, V_{max} should be changed to v_{max}.

p. 270—in the first line S should be changed to s.

p. 270—in the third line under *Transverse bending moment at crown*, V'_{max} should be changed to v'_{max}.

* * * *

The following corrections should be made in "Shear Strength of Reinforced Concrete Beams. Part 1—Tests of Simple Beams," ACI JOURNAL, December, 1954.

p. 320—in the second footnote under Table 3 delete "paste" from the numerator "Volume of paste (Cement + Water)."

p. 327—in the second line *above* the subheading "Behavior after formation of diagonal cracks," change the value of f_c' from 0.40 to 0.040.

* * * *

The following additions should be made in "Static and Fatigue Tests on Partially Prestressed Concrete Constructions," ACI JOURNAL, December, 1954.

p. 362—in Fig. 1, the word "links," which is used in England, means "stirrups."

p. 363—in Fig. 2, the point of the dashed line on the abscissa to the left of the ordinate should be designated A'.

* * * *

The following equations and tabular values should be substituted for the corresponding equations and tabular values in "Ribless Cylindrical Shells," ACI JOURNAL, January, 1955.

$$M_d = \frac{1}{\pi^2/2 - 4}\left[\frac{\pi}{2} - (\pi^2/2 - 4)\,\phi \sin\phi + (\pi^2/4 - 4)\cos\phi\right] p_w r^2 \quad \ldots\ldots\ldots\ldots (9)$$

The changes in Eq. (9) do not affect the corresponding tabular values.

$$M_w = \frac{1}{\pi}\,(\phi\cos\phi - \tfrac{1}{2}\sin\phi)\,p_w r^2 \quad \ldots\ldots\ldots\ldots (10)$$

$$M_T = \frac{1}{6(\pi^2/2 - 4)}\,(2 - \pi\cos\phi)\,\alpha\, TEt^3/r \quad \ldots\ldots\ldots\ldots (11)$$

$$M_T = \frac{1}{3\pi}\,(\cos\phi)\,\alpha\, TEt^3/r \quad \ldots\ldots\ldots\ldots (15)$$

TABLE 1—FIXED SHELL

	V_o/pr	H_o/pr	M_o/pr^2	M_c/pr^2	M_{max}/pr^2	M_{min}/pr^2	
						Value	Location ϕ, deg
p_c	±0.159	∓0.500	∓0.159	0	±0.159	±0.070	37
	$V_o r^2/\alpha TEt^2$	$H_o r^2/\alpha TEt^2$	$M_o r/\alpha TEt^2$	$M_c r/\alpha TEt^2$	$M_{max} r/\alpha TEt^2$	$M_{min} r/\alpha TEt^2$	
						Value	Location ϕ, deg
$\pm T$	0	±0.560	±0.356	∓0.204	±0.356	∓0.204	0, 90

TABLE 2—HINGED SHELL

	$V_o r^2/\alpha TEt^2$	$H_o r^2/\alpha TEt^2$	$M_o r/\alpha TEt^2$	$M_c r/\alpha TEt^2$	$M_{max} r/\alpha TEt^2$	$M_{min} r/\alpha TEt^2$	
						Value	Location ϕ, deg
$\pm T$	0	±0.106	∓0.106	∓0.106	±0.106	∓0.106	90

* * * *

The following corrections should be made in "An Interpretation of Some Published Researches on the Alkali-Aggregate Reaction. Part 1—The Chemical Reactions and Mechanism of Expansion," ACI JOURNAL, February, 1955.

p. 497—below the heading "Introduction," the sixth line, "not" should be changed to "now." In the 14th line, "now" should be changed to "not."

* * * *

The following correction should be made in "Flat Slab Solved by Model Analysis," ACI JOURNAL, February, 1955.

p. 566—The sentence which begins four lines below the heading "Placement of Reinforcing Steel" should read: "Bar sizes and lengths thus determined in Table 1 and the resisting moment of Presan versus M_o reinforcement are shown graphically in Fig. 6b."

* * * *

The following correction should be made in "Strength Variations in Ready-Mixed Concrete," ACI JOURNAL, April, 1955.

p. 766—Eq. (2) change "X" to "$\bar{X}$"

* * * *

The following corrections should be made in "Balanced Design of Prestressed Concrete Beams," ACI JOURNAL, April, 1955.

p. 773—in the sixth line below "Introduction," change "f_{cB}" to "f_{cP}." In Eq. (3) and (4) change "M_S" to "M_L."

p. 774—in the first line from the top change "M_S" to "M_L;" in the second line change "F_o" to "ηF_o."

p. 775—in Eq. (16), change "$f_t Z_t$" to "$f_t Z_b$."

p. 777—in Eq. (a), change "eF_c" to "eF_o." In the second and third lines *above* the heading "Balanced Design with Variable Working Stresses," change "F_c," which appears three times, to "F_o." In Eq. (21), (22), and (23) change all "F_c's" to "F_o."

p. 778—in Eq. (24) change both "F_c's" to "F_o."

p. 779—in the second equation in the last paragraph delete "$\frac{3{,}600{,}000}{2440}$".

p. 780—in Eq. (a) change "eF_c" to "eF_o." In the sixth paragraph, the third and fourth lines, change "...top flange $b_t = 0.7$; $b = 8.39$ in." to "...top flange $b_t = 0.7b = 8.39$ in." and change "...thickness of the rib $b' = 0.5$; $b = 5.99$ in." to "...thickness of the rib $b' = 0.5b = 5.99$ in." In the first line *above* "Conclusion" change "2130 sq in." to "2.130 sq in."

* * * *

The following corrections should be made in "Air Entrainment in Cement and Silica Pastes," ACI JOURNAL, May, 1955.

p. 908—in the ninth line of the second paragraph and in the bold face subheading, "form" should read "foam."

* * * *

The following correction should be made in "Stability of Reinforced Concrete Retaining Walls and Abutments," ACI JOURNAL, June, 1955.

p. 1021—in the fifth line from the top, the sentence beginning "It can be omitted....." should read "It can be omitted practically in all cases where uplift of the heel is not permitted."

* * * *

ERRATUM TO DISCUSSION

The following corrections should be made in the discussion of "Shear Strength of Concrete Beams," ACI JOURNAL, Part 2, Dec. 1955.

p. 732-4—in the second line of the third paragraph, "(d/a)" should read "(d/a')."

p. 732-4—in the second and fourth lines of the last paragraph, each "k'" should read "K'."

p. 732-5—in Fig. C the equation in the ordinate caption, "d/a" should read "d/a'."

p. 732-8—in the second line of the first paragraph, each "k'" should read "K'."

p. 732-16—in the first equation below the figure and in the first line below the equation, "V_s" should read "Vs."

p. 732-20—in the third equation, "V_s" should read "Vs."

p. 732-23—in the second line, "vs" should read "Vs."

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A publication of large format, bound to lie flat
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AMERICAN CONCRETE INSTITUTE 18263 W. McNichols Rd. Detroit 19, Michigan

Discussion of a report by ACI Committee 212:

Admixtures for Concrete*

By W. T. McCLENAHAN, E. W. SCRIPTURE, JR., and COMMITTEE

By W. T. McCLENAHAN†

The report is of much interest and quite instructive, but there is one point with which I wish to take issue. The Sanitary District of Chicago has been experimenting with pozzolans and especially fly ash for the past 20 years, and for several years has been using a fly ash blend with Type II portland cement in all concrete construction. Most of my discussion, therefore, will relate particularly to the use of fly ash, but the same principles will apply to the use of other cementitious materials which the committee classifies as admixtures. My criticism relates more particularly to the classification of pozzolans, natural cement, and other cementitious materials, when used with portland cement, as admixtures. An admixture is defined as being any substance which is added in the mixer other than water, aggregates, and portland cement. Then the committee excludes as admixtures air-entraining cement and blast furnace slag cement. I think this definition is inconsistent and oftentimes misleading. Why is an air-entraining agent an admixture when it is added at the mixer, and not an admixture when it is added at the mill? It is the same thing and does exactly the same thing in the concrete, so why isn't it an admixture wherever it is added?

Another inconsistency is to call a cementitious material an admixture just because it happens to be added with portland cement in the mixer. It is possible to make a good concrete using nothing but fly ash and lime as the cement ingredient. What is the fly ash in this case? Is it an admixture? Or suppose that, to increase the early strength, we decide to add 20 percent portland cement to the above fly ash mixture. Now what is the admixture?

All cementitious materials, whatever they are, should be classified together. We have Types I, II, and III portland cements. Why shouldn't we have pozzolanic and other types of cement blends? Each blend and each type of portland cement will have its own peculiar characteristics with which the engineer should always be familiar. Classifying natural cement, fly ash, and other pozzolans as admixtures leads to a misunderstanding of their function in the mixture.

*ACI Journal, Oct. 1954, *Proc.* V. 51, p. 113. Disc. 51-5 is a part of copyrighted Journal of the American Concrete Institute, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.
†Materials Engineer, The Sanitary District of Chicago, Chicago, Ill.

I have several reports by two different laboratories which give the proportions that were used in some experimental work. In both cases the materials used are listed in the following order: (1) portland cement, (2) fine aggregate, (3) coarse aggregate, (4) fly ash, (5) Darex, (6) Plastiment, and (7) water. Both laboratories treated the fly ash simply as an admixture; indeed, the committee's definition of an admixture would warrant them in doing that. That this definition is misleading is brought out further by the way one of the laboratories reports the water-cement ratio; it is given as gallons per sack (based upon the portland cement only). Then, apparently as an after thought, this laboratory does state the water-cement ratio "by weight of cementitious material."

Neither of these methods of stating the water-cement ratio is correct. In the first place, the water-cement ratio should be stated as gallons per sack of cementitious material, a sack of fly ash being computed as having the same absolute volume as does a sack of portland cement. Then the strengths will be truly comparable on the basis of the water-cement ratio. The other way of stating the ratio is equally wrong. Suppose the proportion of fly ash in the blend is changed—the fly ash being lighter than the portland cement throws the relationships out of kilter. We cannot accurately compare the water-cement ratio of a blend with that of straight portland cement on a weight basis. They are truly related only when expressed by volume.

In stating proportions I always like to give them in the following order: (1a) portland cement, (1b) fly ash, (1c) total cementitious, (2) fine aggregate, (3) coarse aggregate, (4) water, and (5) air-entraining agent. In that way the true significance of the materials in the blend is brought out.

Let us now consider another mistake that derives from this method of considering the fly ash to be an admixture. In our work we consider 75 lb of Chicago fly ash to have the same absolute volume as 94 lb of portland cement. The weight of fly ash varies somewhat, depending on its composition —lighter where the carbon runs high, and heavier where the iron content is high. A suitable unit weight needs to be determined in each case, of course, but we have found that the above unit weight is reasonably satisfactory for Chicago material.

Now, if in a 6-bag mix we take out 1 bag of portland and add 75 lb of fly ash, we make no change in the absolute volume of the cementitious material; the mix will still be 6 bags. If, however, we make the substitution on a weight basis, *i.e.*, add 94 lb of fly ash for the 94 lb of portland cement removed, we actually increase the cementitious material in the mix by about a quarter of a bag, so that instead of a 6-bag mix we now have a 6¼-bag mix. We have actually increased the cementitious material. In like manner if we should substitute 150 lb of fly ash for the 94 lb of portland cement taken out, we would increase the cement content to 7 bags.

We cannot accurately consider the extra volume of fine material to be admixture because it still acts as cement and therefore enriches the mix.

Of course, because of the slowing up of the hydration process by these substitutions of fly ash, the extra volume of cement may be warranted to assure sufficient strength when the forms are removed, or because the conditions for hydration soon become unfavorable. I firmly believe, however, that the engineer should treat the fly ash material as cement and be fully aware of why he is increasing the cement volume.

To recapitulate, I believe that all cementitious materials should be classified and handled together as cement. Only then will the initiate fully understand what he is doing. Air-entraining agents should be considered admixtures wherever they are incorporated. On the same basis, the calcium sulfate which is added at the mill should be considered to be an admixture because it is not cementitious but is used to govern the rate of hydration.

The criterion should be: is it or is it not cementitious when used in the mix?

By E. W. SCRIPTURE, JR.*

The committee is to be complimented on the thoroughness with which this subject has been presented. There are only a few items which might benefit from further clarification.

In the list of the objectives of the use of admixtures the committee could have listed reduction in unit water content. It might be considered unnecessary to list this since the end result is indicated by improvement of the properties which are listed, such as increase in durability, reduction in bleeding, and many others. Reduction in unit water content, however, is such an important item that it could well deserve to be listed separately.

The statement that the specific effects which will result from the use of an admixture can seldom be predicted accurately, is undoubtedly correct but it is perhaps unnecessary and somewhat misleading. It appears to condemn all admixtures categorically, including air-entraining admixtures. The committee points out elsewhere that it is desirable to base the evaluation of a given material on the results obtained with the particular concrete in question. This is certainly sound practice, but these precautions are equally applicable to the cement, sand, coarse aggregate, water, and any other materials which it is proposed to use in concrete. The statement seems to single out admixtures as being especially unpredictable, which is questionable.

The section of the report on retarders, under the general heading "Admixtures used for grouting," (p. 136) contains a number of trade names. It is not explained in this connection that most of the materials so named are actually calcium lignosulfonate, or derivatives thereof.

The use of entrained air in grouts has been well covered by the committee, except that a word of caution might have been added. On one dam job considerable difficulty was experienced in pumping a grout in which an air-entraining cement was used. It is understood that it was a neat cement grout and that it eventually built up a slug of air in the grout pump. From

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then on all the pump did was to compress the air on the compression stroke, relieve the compression on the back stroke, and pump no grout.

In discussing admixtures for dampproofing and permeability reduction the committee mentions the use of soaps. It does not mention an emulsifiable stearic acid material which, strictly speaking, of course, is not a soap but is used in the same way as soaps and becomes calcium stearate in the concrete. In this section the discussion of workability agents mentions improved placing properties and the entrainment of some air but does not emphasize the substantial reduction in unit water content which can be secured with these agents. Such reduction in unit water content should decrease permeability, and reference No. 37 given by the committee gives data to support this expectation.

The section on water-reducing agents appears to cover these admixtures in a satisfactory fashion, except that it fails to bring out adequately the importance of water reduction and subsequent improvements in the concrete or reduction in the cost of the concrete. While the section on air-entraining agents mentions the water reductions possible with such materials, the section on water-reducing agents does not point out that substantially greater reductions in unit water content can be secured with certain water-reducing agents than can be secured with simple air-entraining agents.

Under the section headed "Miscellaneous," a material consisting of powdered iron plus an oxidizing agent is described. Actually, the term "oxidizing agent" is not correct chemically. It should be called a "catalyst" or "oxidation catalyst." This description overlooks the fact that the oxidation of iron is a reversible action under pressure, and that if the expanding grout is confined, later growth will cease if the load is continuously maintained.

COMMITTEE'S CLOSURE

Mr. McClenahan questions, in particular, the committee's classification of pozzolans and, in general, its classification of admixtures. Probably this matter of his concern is more than a problem of definition and use of names, since the way in which the names are used and the confusion that exists in their use is merely a reflection of the confusion that exists in the use of the materials so designated. We believe that what is really needed is a greater clarification of the significance of the use of admixtures and that when this clarification is achieved the problem of terminology will automatically arrange itself.

As stated in the report, the committee adopted as a definition for an admixture that given in the present ASTM Designation C 125. The terminology having been settled, the report then followed an established pattern. Responsibility for development of definitions relating to concrete and concrete aggregates is that of Subcommittee I-d of ASTM Committee C-9, which functions as a joint committee on definitions and nomenclature in the field of concrete with ACI.

Mr. McClenahan also refers to the proportioning of concrete mixtures containing certain types of admixtures. In this connection "Recommended Practice for Selecting Proportions for Concrete (ACI 613-54)," ACI JOURNAL, Sept. 1954, *Proc.* V. 51, pp. 49-64, provides the most up-to-date recommendations. This standard explains that almost any change in the ingredients will necessitate a redetermination of the proportions if properties of the concrete are to remain unchanged. For example, adequate comparisons between concrete mixtures which contain pozzolan and related mixtures which do not contain pozzolan can be made of significant concrete properties in terms of both weight and solid volume of the ingredients per unit volume of concrete and their water-cement ratios. But it should be noted that:

1. If pozzolan is used solely as a material to correct for deficiencies in fine aggregate grading, the fact that it is also cementitious in effect is relatively incidental.

2. If pozzolan is used as a cement-replacement material by substituting it for an equivalent solid volume of portland cement, and a constant ratio of water to cementitious material by solid volume and a constant slump are maintained, then the volume of cement plus pozzolan per cubic yard of concrete will have to differ from the volume of cement per cubic yard of concrete when no pozzolan was used, since a change in the cement factor will be required to compensate for the difference in effect on slump of pozzolan as compared with cement.

3. If the quantity of cement plus pozzolan by absolute volume and the slump are maintained the same, then the ratio of volume of water to volume of cement plus pozzolan will have to change.

4. Finally, if one substitutes for a given solid volume of portland cement a larger solid volume of pozzolan, maintaining a constant slump, their original values of strength, cement factor, or water-cement ratio as a basis of the mixture proportioning will be modified.

Mr. Scripture's discussions do not take issue with any particular portions of the report. Rather, they are ideas evidently presented to clarify or strengthen the report. First, he feels that the reduction in unit water content due to certain admixtures is such an important item that it should be listed separately. Certainly the committee cannot take issue with this statement. Second, it was not our intention to single out admixtures as being especially unpredictable; rather, throughout our report we have attempted to emphasize the need and desirability of testing the given admixture in combination with the other ingredients for a given job prior to use. As Mr. Scripture states, most of the material listed in the subparagraph on retarders under "Admixtures used for grouting" are actually calcium lignosulfonate or derivations thereof. Mr. Scripture's caution on the use of air-entraining agents in pumping neat cement grouts, and the use of an emulsifiable stearic acid as a dampproofing and permeability reduction agent, are worthy of note.

The committee is indeed grateful for the discussions. We are also fully

cognizant of the fact that many aspects of the technology of admixtures have been developing at an exceedingly rapid rate. For this reason, the committee, to conduct its considerations of these matters with the greatest possible efficiency, has subdivided itself into the following Task Groups: (1) Accelerators, (2) Air-Entraining Agents, (3) Retarders, (4) Alkali-Aggregate Expansion Inhibitors, and (5) Dampproofing and Permeability Reducing Agents.

In addition we have organized a nonvoting advisory committee composed of representatives of producing interests to aid the committee in its deliberations and activities and to preview future reports of the committee.

With regard to the work of the task groups, Task Groups 2 and 4 are even now occupied in considering needed modification of the material contained in the present report.

Research studies in process, as well as recent important technical developments on air-entrained concrete, must be considered further. Task Group 2 is following results of current research and field observations, and will recommend changing the committee's report on air-entraining agents when the need becomes apparent.

Task Group 4, confronted with the question of the use of admixtures for preventing or reducing the deleterious expansion caused by alkali-aggregate reaction, finds itself wondering about the fundamental chemical phenomena involved in the pozzolanic reaction and the alkali-aggregate reaction. Research results that have come recently to the attention of the task group suggest that perhaps the term "inhibitor," as used in the report, has connotations that are not correct. The task group has suggested the desirability of reviewing these fundamental considerations with a view to simplification of the concepts involved. One working hypothesis that may help to direct future study is that the same participants are involved in the chemical phenomena that are known respectively as "alkali-aggregate reaction," "pozzolanic reaction," and "alkali-aggregate expansion inhibition." An interpretation of data on the chemical reactions and mechanism of expansion in alkali-aggregate reaction has recently been presented.* Similar interpretations of pozzolanic reactions and of the relations between both and the function of admixtures designed to prevent such an expansion will permit the task group to develop an improved, simplified report. It may be that differences between admixtures in the effectiveness with which they prevent alkali-aggregate reaction will be measured by the same procedures that measure their effectiveness as pozzolans and that this effectiveness will be found to depend on their surface area, their silica content, and relative reactivity of the silica.

*Powers, T. C., and Steinour, H. H., "An Interpretation of Some Published Researches on the Alkali-Aggregate Reaction. Part 1—The Chemical Reactions and Mechanism of Expansion," ACI JOURNAL, Feb. 1955, *Proc.* V. 51, pp. 497-516.

Discussion of a paper by F. Thomas Collins:

Precast Concrete Sandwich Panels for Tilt-Up Construction*

By WILLIAM R. LORMAN and AUTHOR

By WILLIAM R. LORMAN†

On the basis of comparative size of the wall panel in Fig. 1, the writer is inclined to believe that this 1906 structure was a commercial or an industrial type. It is of interest, therefore, to note that one of the earliest instances of precast tilt-up residential construction was that described in 1916; the construction involved precast plaster cores to decrease weight and to air-space the wall panels which were 10 to 20 ft long and one story high.‡

The first North American structure incorporating wall panels comprised of a concrete sandwich with a cellular-glass core was an industrial warehouse erected in 1947 in the Province of Quebec, Canada. Forrestal Village, erected in 1951-52 at Great Lakes, Ill., was the first residential housing project in North America to employ concrete-glass sandwich construction.

The section entitled "Cellular glass sandwich panels" contains information slightly in error relative to fabrication of the wall panels at Forrestal Village.§ The 4 in. thick inner shell of the concrete wall panel was cast in two layers. Flat sheets of precut wire fabric (4 x 4-in., No. 4/No. 4) were laid on the initial layer of 2-in. plastic concrete; the wire fabric was not placed on chairs. On reaching the end of the wall panel resting on the casting bed, the concrete crew doubled back immediately to place the second 2-in. layer of the inner shell; contrary to Mr. Collins' description, the concrete was not placed in one operation to approximately half the thickness of the walls. As only several minutes elapsed between placement of the first and second layers of concrete, the shell virtually became monolithic. The first layer was compacted internally with 2-in. diameter vibrator heads.

The original type of shear tie, used during the early period of construction at Forrestal Village, was a 6 in. long J-shaped steel pin. This pin method entailed not only punching through the Foamglas but filling the resultant hole with portland cement grout and reinserting the pin. On the average,

*ACI JOURNAL, Oct. 1954, *Proc.* V. 51, p. 149. Disc. 51-6 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

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The opinions or statements in this discussion are those of the writer, and are not to be interpreted as an expression of official U. S. Navy policy nor are they necessarily a reflection of the official views of the U. S. Naval Civil Engineering Research and Evaluation Laboratory, Port Hueneme, Calif.

‡"A Simple Apparatus Raises Side of Concrete House," *Concrete*, V. 8, Jan. 1916, pp. 41-42.

§Lorman, William R., and Wiehle, C. K., "Assembly-Line Technique Results in Low-Cost Concrete Construction at Forrestal Village," *Civil Engineering*, V. 23, Nov. 1953, pp. 760-764.

one pin was employed for each 12 x 18 x 1.5-in. glass block. The J-pin method soon was superseded by a method involving the use of 4 in. wide strips of 4 x 4-in., No. 9/No. 9, welded wire mesh. The expanded metal shear ties described by Mr. Collins, using No. 6/No. 6 wire fabric, later were found more desirable than those of No. 9/No. 9 wire. During the early stages of the project, severe cracking was encountered in the erected wall sections at re-entrant angles formed by openings such as doors and windows; subsequently, such corners were reinforced with an extra layer of 2 x 2-in., No. 10/No. 10, wire fabric which was fastened to the primary reinforcement.

In the section entitled "Outer shell design," the desirability of an admixture to improve concrete workability is mentioned as a practicable feature. The writer would like to emphasize that air-entraining agents not only would insure greater workability at time of concrete casting (with resultant decrease in bleeding and laitance) but, especially vital in geographical areas such as Forrestal Village, air entrainment would increase appreciably the resistance of the wall panels to alternate cycles of freezing and thawing. As the exterior portions of the sandwich type wall panel also may be subject to severe deterioration caused by alternate wetting and drying, it appears that consideration also should be given to the use of vacuum curing or high-pressure-steam curing to minimize swelling and shrinking tendencies incident to wetting-drying cycles. Because of autoclave size limitations, however, the large wall panel Mr. Collins had in mind would require considerable modification if high-pressure-steam curing were employed. While it is true that more rapid erection of wall panels, with the added advantage of ease in handling, would be assured by using high-early-strength portland cement in combination with lightweight aggregate, the additional expense of hauling special aggregates from distant sources might not justify their use.

It is apropos to note that a fire resistance test of a 6 in. thick expanded slag concrete sandwich panel (encasing a 2 in. thick Foamglas) was conducted at the National Bureau of Standards in February, 1950. A fire resistance rating of 4 hr was assigned to this particular assembly; all requirements of the hose stream test were fulfilled by the test panel after 6 hr of fire exposure where the maximum temperature was about 2250 F. As far as known, no other fire endurance investigations of this type sandwich construction have been made since 1950.

In regard to the section entitled "Prestressed sandwich panels" and Fig. 16, the writer would like to state that, as far as known now, no investigation has ever been made concerning racking load capacity of a typical concrete-glass-concrete sandwich wall panel. However, flexure and shear properties of such panels were investigated in 1948 by L'Ecole Polytechnique at Montreal, which also performed pull-out tests for bond of a metal anchored in the panel.

AUTHOR'S CLOSURE

Thanks are extended to Mr. Lorman for his corrections to the paper and

for the additional information presented on sandwich wall panel design and construction.

The areas in the United States that could make the greatest use of sandwich wall panels such as described in this paper are still using the more conventional and old fashioned methods of construction that have been handed down for centuries. When builders in the north and north eastern regions of the United States do adopt the successful tilt-up methods used by builders in the south and southwest, and discover the advantages, a great deal more interest in sandwich wall panel design and fabrication will be developed than shown heretofore.

It will be interesting to note too, perhaps, the increased interest in tilt-up sandwich wall panels as the building codes of each locality tighten up their requirements on concrete walls as regards fire resistance. There is a definite tendency toward a universal 8 in. thickness instead of the 6 in. allowed in the past. It is believed that tilt-up wall panels, of 5 in. thickness and of sandwich design, will be the economical answer to the 4-hr wall requirement.

The prestressed sandwich wall panels of 8 ft width and in lengths up to 50 ft should find a definite place in multistory construction. Such panels could make use of steam curing as described by Mr. Lorman. Pre-tensioning the wall panel in the longitudinal direction on a central prestressing casting bed, hauling it to the job on a truck bed, and then post-tensioning in the horizontal direction (largely as a means of joining the wall panels) is suggested.

Discussion of a paper by E. M. Zwoyer and C. P. Siess:

Ultimate Strength in Shear of Simply-Supported Prestressed Concrete Beams Without Web Reinforcement*

By A. J. ASHDOWN, ORESTE MORETTO, I. M. VIEST, and AUTHORS

By A. J. ASHDOWN†

Messrs. Zwoyer and Siess are to be congratulated on writing a paper with so much detail on a subject about which so little is known. The computations appear to be based upon parameters which are somewhat cyclical, some being obtained from the tests themselves.

With regard to the observation on page 184, "that shear failure could be considered as a type of premature compression failure resulting from the greater height reached by the inclined crack as compared to the vertical cracks caused by flexural stresses"—this conclusion does not entirely agree with my experience. The inclined shear cracks certainly do invade the zone of compressive depth required for flexure, but recent tests here on ordinary reinforced concrete beams show that shear failure can occur before crushing of the concrete, and when a horizontal crack appears at the level of the main reinforcement, or along the soffit of the beam parallel to it and near the end of the beam. The latter crack was formed in every beam failing by shear and could be easily overlooked. This horizontal crack is due to the failure of the concrete in circumferential tension, caused by wedge action between the steel and the concrete as the intensive bonding action develops at failure.

In a recent test, and to prevent circumferential tension failure, wire coils were introduced around the bars near the ends of one of two otherwise identical beams cast from the same batch of concrete. The beam with the wire coils did not prevent the usual diagonal crack from forming and opening; in fact, it opened up so much that lumps of concrete fell out. Failure occurred when the concrete suddenly sheared horizontally over the top of the coils. This beam carried 21 percent greater load than the beam without the coils.

In another beam reinforced with a few vertical stirrups, failure occurred when a horizontal crack opened along the soffit. In all cases the compressive strains were small.

*ACI JOURNAL, Oct. 1954, *Proc.* V. 51, p. 181. Disc. 51-8 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

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In post-stressed beams with cores which are grouted after elongation of the wires, the treatment of the grout to secure concrete bonding of the steel and of the core and to eliminate shrinkage effects is of great importance since if it is not so treated, the ultimate bending moment of a beam failing by flexure will be reduced by some 30 percent. Since flexure failure occurs so early, failure by shear will not be evident in such beams.

Rusting of the prestressing wires to improve the bond is not necessary since deformed high tensile wires are now commercially available at little extra cost. The ultimate breaking strength is actually improved in the deforming process.

The values of k_1k_3 obtained from the formula $k_1k_3 = 1.500 - 0.715 \times 10^{-4}f_c'$ are extremely high. For instance, if $f_c' = 2500$ psi, $k_1k_3 = 1.32$, which means that if k_1 is as high as 1, the maximum stress must be considerably higher than 3300 psi, a state of stress which is difficult to believe. The truth possibly lies in the fact that both above and below the top of the inclined crack the resultant compression is inclined parallel to the crack, toward the support.

Whatever method may eventually be adopted for calculation of the shear failing load, there is bound to be a big scatter of experimental results since ultimate failure is dependent primarily on tensile strength of the concrete.

The ratio of shear span to depth is a big factor affecting shear resistance. The effect of prestress is to modify or reduce the effective shear span. The final horizontal bond resistance to shear comes into effect where the surrounding concrete is tensioned by flexure. Tension in the concrete occurs at a distance from the support where the effect of prestress in the concrete is nullified by the bending moment, at or before the position nearest to the support where the concrete can be expected to crack due to excessive tensile stress.

It is true that so-called "bond failure" and "circumferential tension" failures may be confused. Actual slip of smooth bars may take place without any external signs, leaving the bars, as it were, "hanging by the hooks."

By ORESTE MORETTO*

The paper presents a new and original approach to the analysis of the ultimate strength of prestressed and reinforced concrete beams failing in shear. In it the ultimate capacity of the beams at failure is not expressed as a function of the stresses produced by shear forces, but rather as a function of a secondary effect originated by these forces.

When shear forces are predominant, failure of a simple beam originates as a consequence of the inclined cracks produced by the principal tension stresses. As explained by the authors, the inclined cracks are only instrumental in developing an early flexural failure. The mechanism of this failure is easily explained if the state of equilibrium of the beam along a section AB is studied (Fig. A).

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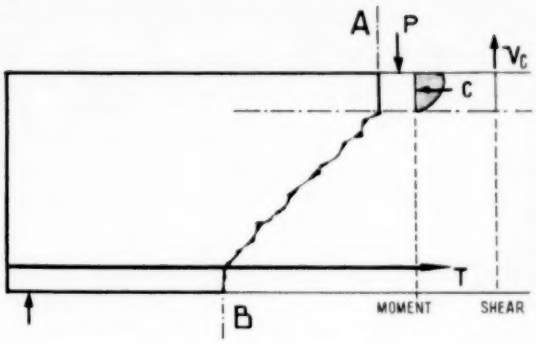
Fig. A—Equilibrium in a beam without web reinforcement



Section AB is subjected to a shear V and a moment M. In a beam without web reinforcement, there are no shear forces along the crack and all the shear is supported by the compression zone of the beam, where distribution of shear stresses must be similar to that indicated in Fig. A. (It is assumed that the main reinforcement cannot take shear.) A study of the Mohr rupture line for the concrete shows that it is materially impossible to produce a pure shear failure of the concrete. Since, at and near the neutral axis the normal stress is small, the principal tension stress is large and it overcomes the concrete resistance and opens the crack upward. However, at points far from the neutral axis, the normal stresses sharply decrease the principal tension stresses, barring the possibility of reaching a state of rupture either by tension or shear. Consequently, final failure of the beam can only occur, as explained by the authors, by overcoming the simple compressive strength of the compression zone of the beam.

In a beam with web reinforcement, the distribution of forces along the composite section AB is slightly different. In this case, the main inclined crack can only open up after the web reinforcement has been overstressed (in bars with a yield point, after yield point has been reached). Equilibrium requires that (see Fig. B): $V = V_c + V_s$—in which V_c is that part of the total shear taken by the compression zone of the concrete, and V_s that taken by the stirrups. Presence of the forces V_s makes the moment acting on AB smaller than the one existing in the vertical section passing through A. The analysis made by the authors for the reinforced concrete beams with web reinforcement ignores the existence of the forces V_s on the section of failure.

There are several points which the writer would like to have clarified. These are:

1. Values of k_1k_3 appear, as indicated in Fig. 5, to be consistently much larger than one. This signifies that the volume of the compression stress block of the beam at failure is larger than the value obtained with a rectangular block, multiplying the width b of the beam by $k_u d$ and by the cylinder strength of the concrete f_c'. This result seems to be in contradiction with existing experience of beams failing in flexure.

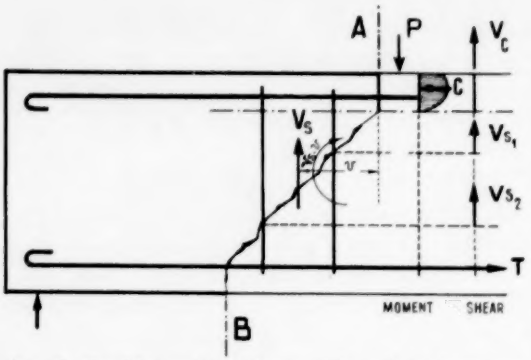


Fig. B—Equilibrium in a beam with web reinforcement

2. The ratio of the nominal shear stress at failure to the compressive strength of the concrete appears to be unusually low. The authors state: "Before cracking of the concrete, all of the beams showed essentially elastic behavior." If such was the case, theoretically, cracking should not have started until the principal tension stress became equal to the concrete tensile strength. Results of the tests, however, appear to indicate that, in most cases, failure of the beams occurred at stresses that were only a fraction of that value.

3. The analysis made by the writer (Fig. B) shows that, in beams with web reinforcement, the moment producing failure should be smaller than the value taken by the authors in their analysis. This difference is equal to the moment of V_s with respect to the point of application of the compression force C. Yet the results obtained by the authors give an excellent correlation between calculated and experimental values. It makes one wonder whether that correlation is not just the consequence of a forced selection of the coefficient k_1k_3 so as to fit results.

4. Finally, in beams with web reinforcement, the writer believes that any analysis of its behavior should endeavor, if possible, to separate neatly the different parts of the shear strength taken by the various elements of the beams, at different stages of loading if necessary. The reason for this need lies in the difference in quality of the two materials that enter in the construction of reinforced concrete and on the bearing that this fact has on the factor of safety to be selected in design. One can rely on the uniformity of the quality of steel much more than one can on that of concrete. For this reason, a factor of safety of 1.5 or 2.0 may be amply adequate for steel, while for concrete a minimum of 2.5 is usually called for.

The authors are to be commended for the presentation of a new way for the analysis of a problem that has proved to be one of the more difficult in the theory of reinforced concrete. Supplemented now by an extensive and systematic investigation under way at the University of Illinois, as mentioned by the authors, it should yield a solution that would lead to its complete understanding.

By I. M. VIEST*

After decades of unsuccessful attempts to express the shear strength of reinforced concrete beams in terms of the nominal shear stress at failure, it is gratifying to see that the authors embarked on a new approach consistent with experimental evidence. It is also encouraging that the limiting shear-moment concept developed for the prestressed beams was found by the authors applicable to ordinary reinforced concrete beams thus underlining the basic identity of prestressed and reinforced concrete.

This writer would like to point out, however, that a concurrently published study of the shear strength of simple and restrained ordinary reinforced concrete beams† has shown that the limiting shear moment alone may not be sufficient for predicting the load at shear failure of all beams. In beams with small ratio of shear span to effective beam depth, failure occurs at the limiting shear moment after an inclined crack formed at lower loads. Beams with very large ratios of shear span to effective depth fail in flexure. The transition between these two types of failure appears to be connected with the formation of inclined cracks. Beams with moderately large ratios of shear span to effective depth seem to fail simultaneously with the formation of the first major inclined crack. For such beams, the shear-moment equation may yield a lower ultimate load than that actually attained. Thus to evaluate the ultimate shear strength of a reinforced concrete beam, it may be necessary to determine two quantities: (1) the load at which a major inclined crack forms and (2) the load corresponding to the limiting shear moment.

The authors state that "...the shear-moment expression applies only to beams in which inclined cracks have developed to such an extent that the capacity of the compression zone of the beam is impaired." In other words, the presence of a major inclined crack is a prerequisite for failure at the limiting shear moment. Thus it seems reasonable to expect that under some conditions the type of failure discussed above for ordinary reinforced concrete beams may occur also in prestressed concrete beams.

The studies of the shear strength of reinforced concrete beams were hampered by the lack of quantitative information on formation of inclined cracks. Since most investigators reported only data on the ultimate load, it is impossible to determine whether the beams failed at the formation of inclined cracks or by exceeding the limiting shear moment. To avoid a similar situation occurring in the studies of shear failures of prestressed concrete beams, the authors are urged to include in the closing discussion some quantitative information on the formation of major inclined cracks.

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†The results are reported in a series of four papers entitled "Shear Strength of Reinforced Concrete Beams," ACI JOURNAL, Dec. 1954-Mar. 1955, *Proc.* V. 51, pp. 317-332, 417-436, 525-540, 697-732.

AUTHORS' CLOSURE

It is evident that the discussors have given much thought to the question of strength in shear and have drawn on their own considerable experience in research in the preparation of their discussions. The authors are particularly pleased, therefore, that no serious objections have been offered to the new concept of shear failure that was presented in the paper or to the moment-failure type of analysis that was developed to predict the strength of beams in shear.

Both Mr. Ashdown and Dr. Moretto have raised some question regarding the empirical evaluation of the parameters. Mr. Ashdown refers to this process as "somewhat cyclical" and mentions that some of the parameters were "obtained from the tests themselves," while Dr. Moretto refers in one place to a "forced selection . . . . to fit the results." The writers must plead guilty to doing exactly this, but believe that the experimental evaluation of certain parameters appearing in rationally derived expressions is a legitimate part of the empirical solution that was attempted. Criticism, if any, would more properly be directed at the use of an empirical rather than a completely rational solution of the problem. This would have been preferred by the authors but was not possible in view of the limited scope of the tests and available data.

Mr. Ashdown mentions a type of failure involving a horizontal crack at the level of the reinforcement which he has observed in tests on ordinary reinforced concrete beams. This type of "splitting" failure has also been noted by the authors in tests of ordinary reinforced concrete beams, and occurred also as a secondary phenomenon in the tests of prestressed concrete beams as can be seen from Fig. 3. In the case of ordinary reinforced concrete beams, it is not clear that this type of failure is due solely to wedging action of the reinforcement; there is at least a possibility that it may be the result of stresses produced by dowel action of the bars. Moreover, in many cases, splitting has appeared to be a secondary effect and the failure load could be predicted satisfactorily by the equations for moment. In any case, the data presented by Mr. Ashdown indicate clearly the need for further study of this type of failure.

The importance of good bond, as pointed out by Mr. Ashdown, was fully appreciated at the time these tests were planned, and is in fact being studied at the present time by tests of unbonded beams. The use of aluminum powder in the grout and the rusting of the wires represented attempts to insure adequate bond and thus to eliminate this variable from the series of tests reported. Unfortunately, deformed high tensile wires are not yet commerically available in the United States although they have been produced on an experimental basis.

Both Mr. Ashdown and Dr. Moretto have commented on the high values obtained for the parameter k_1k_3. It is entirely possible, even probable, that the high values of k_1k_3 may be accounted for in part by the fact that a portion of the resultant compression is carried by the concrete below the top of the

inclined crack. Evidence to support this view has been obtained from a current series of tests on ordinary reinforced concrete beams. However, if this should be the case, the conditions of statical equilibrium require that a portion of the shear be resisted by dowelling action of the reinforcement at the lower end of the crack.

The fact that the values of k_1k_3 given in Fig. 5 tend to be somewhat higher than those given by Billet and Appleton* from tests on beams failing in flexure further supports the view that the values of the average compressive stress, $k_1k_3f_c'$, may be unduly high because of an erroneous assumption as to the true depth of the compression zone. Nevertheless, values of k_1k_3 greater than one should not be unacceptable since there is no reason why the maximum stress developed in a beam should be limited by the strength of a standard test cylinder. This matter has previously been discussed by Billet and Appleton in their closure to the discussions of the paper referred to above.†

Although the authors agree enthusiastically with Mr. Ashdown's comment regarding the inevitability of scatter, they cannot agree fully with his statement that "the ultimate failure is dependent primarily on the tensile strength of the concrete." As discussed by Dr. Moretto, it seems more likely that the final destruction of the compression zone is related to the compressive strength of the concrete or to some function of its strength under combined compression and shear. Some of the differences of opinion expressed may be the result of our confusing terminology; for example, the "compression" failure of a standard test cylinder or cube is actually a shearing failure on inclined planes, and may even be considered by some to be a tension failure on certain planes.

Dr. Moretto's question concerning the relation between cracking stress, principal tensile stress, and nominal shear stress is not entirely clear. It may help, however, to point out that flexural cracks appeared first in all cases and that the tensile stresses in the bottom of the beam at first cracking compared favorably with the modulus of rupture obtained from tests on standard beams. At failure, the nominal shear stress v, as well as the principal tensile stress S_t, computed by conventional methods bear no relation to the actual stresses in the cracked beams.

Dr. Moretto's comments in items (3) and (4) regarding the neglect of the moment resistance contributed by the web reinforcement apply presumably only to the studies of ordinary reinforced concrete beams since the prestressed beams were not provided with web reinforcement. A completely rational approach to this problem would require separate consideration of the moment resisted by the web reinforcement, and this has been done by Moody and Viest in their comprehensive study.‡ However, because of the limited scope of the

*Billet, D. F., and Appleton, J. H., "Flexural Strength of Prestressed Concrete Beams," ACI JOURNAL, June 1954, *Proc.* V. 50, pp. 837-854. See Fig. 8(a), p. 849.
†ACI JOURNAL, Dec. 1954, Part 2, *Proc.* V. 50, pp. 856-12-856-14.
‡Moody, K. G., and Viest, I. M., "Shear Strength of Reinforced Concrete Beams. Part 4—Analytical Studies," ACI JOURNAL, Mar. 1955, *Proc.* V. 51, pp. 697-732

study of ordinary reinforced concrete beams in this paper, no attempt was made to consider the contribution of the web reinforcement separately and all of the effects resulting from its presence were simply grouped in the empirical expression for the parameter $K\epsilon_u$ in Fig. 10. Thus it might be said that there was a "forced selection" of $K\epsilon_u$ rather than of k_1k_3 which was simply assumed to be the same as for the prestressed beams. As pointed out in the paper, this procedure is open to question and its validity can be judged only in terms of the results which admittedly involve too few tests to be conclusive.

The authors agree with Dr. Moretto regarding the desirability of separating the contributions of the steel and the concrete so that factors of safety can be assigned more intelligently.

The effect of the shear span–depth ratio on the formation of cracks and on the mode of failure has been discussed by Mr. Ashdown and Dr. Viest. Dr. Viest has postulated that failure simultaneously with the formation of the first major inclined crack might occur in beams having a/d ratios intermediate between the small values, for which the failure would be like that described in the paper, and the large values, for which failure would occur by flexure before inclined cracks could develop. Failures of this type in the transition range of a/d have been observed for ordinary reinforced concrete beams in the tests mentioned by Dr. Viest as well as in tests recently carried out by one of the authors.

The prestressed concrete beams tested had a/d values ranging from 2.8 to 6.7 (Table 1); none of the beams failed by the sudden formation and propagation of an inclined crack. The inclined cracks usually formed as an extension of an existing flexure crack and, with increasing load, extended upward and toward the load, usually becoming almost horizontal before final failure by crushing occurred. This type of behavior was especially typical of the beams having the medium to long shear spans. However, in the beams with the shortest shear spans, final failure was accompanied by the more or less sudden formation of a new inclined crack.

For beams with effective prestresses in the range of 100-140 kips per sq in., the first inclination of the cracks occurred at loads ranging from 75 to 97 percent of the failure load and at deflections varying from 33 to 72 percent of the deflection at failure. No variation of this percentage as a function of a/d was observed for a/d values ranging from 2.8 to 6.7.

Although the formation of cracks has been restudied as requested by Dr. Viest, it seems that it is still true, as stated in the papers, that these tests provide no data regarding the effect of moment-shear ratio (a/d) on the development of inclined cracks and the final mode of failure. Additional studies involving prestressed beams having a greater range of a/d are now being planned in an attempt to investigate the possibility of sudden failures in the transition range.

Attention is called to two numerical errors in the original paper: In lines 1 and 2 on page 182, the dimension "15 in." should read "18 in." The values of a/d given in Table 1 are correct, however.

Discussion of a paper by D. H. Pletta and D. Frederick:

Model Analysis of a Skewed Rigid Frame Bridge and Slab*

By PAUL I. RONGVED and AUTHORS

By PAUL I. RONGVED†

This was an interesting report on laboratory work with models for skewed slabs and slab-frames. It is hoped that this will be the beginning of the necessary work that ACI must perform in this field to bring accurate and more satisfying calculation methods for skewed slab and slab-frame bridges within the reach of most designers of the present and future large highway construction program.

No attempt was made to tie this specific model testing to a more general solution of the problem, but this is almost a must for the practical designer. The writer has worked a great deal on skewed-slab and slab-frame problems and will in the following give a brief picture of the present possibilities for analytical solutions to these problems.

The general differential equation for the slab was set up about 150 years ago. Since then many scientists and engineers have found various solutions to these problems; today we have literally thousands of publications giving the solutions of slab problems. The analytical slab solutions, especially those for skewed slabs, are quite involved and it would not be advisable to use these solutions in their normally presented form for everyday needs. It is, however, possible to eliminate the involved higher mathematics from these solutions by presenting them in tabular or graphical form. When presented in this form, even a slab problem with complicated loadings and edge conditions can be solved accurately and quickly by any structural designer.

The accurate solutions show, especially for larger spans, a tremendous saving compared to methods presently used for slab design, in some cases bending moments for live load are reduced to less than 40 percent. At the same time the accurate solutions make larger spans possible and economical, and an over-all saving on bridges through dead load reductions and a more uniform safety factor throughout the structure can be achieved.

The basis for this modern form of slab calculation is the "influence fields" that are used for all types of slabs with any type of loadings in the same way as influence lines are used for beams. These influence fields were de-

*ACI JOURNAL, Nov. 1954, *Proc.* V. 51, p. 217. Disc. 51-9 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

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veloped[1] in the 1930's for rectangular slabs and are found in various publications.[2,3] This theory for rectangular slabs has now largely replaced the old methods and codes for highway slabs in Europe that were similar to those often still used here; this change accounts for large savings in their newer bridges.

In the 1940's the writer developed a general method for calculation of skewed slabs.[4] The basic solutions for known influence fields can be used for all shapes of slabs, if the right homogeneous equations are added to satisfy the specific new edge conditions. This theory can also be used for calculation of influence fields for skewed slab-frame bridges by using the integral of the partial restrained edge moments as redundants.

This theory is at present used for tabulation of skewed slabs for the new code of the German Highway Authority under Professor Rusch and his research staff in Munich, Germany, and also recommended in some of their latest publications.[5] There is a need for similar tabulation of influence fields in this country, and with so much work already done in this field it should not be too hard to get started on this work. Certain experimental methods could also be helpful in facilitating this.

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3. Bittner, E., *Momententafeln und Einflussflaechen fuer kreuzweise bewehrte Eisenbetonplatten*, Vienna, 1938 (a new edition is being prepared by Springer-Verlag).

4. Rongved, P. I., *Ueber das Randwertproblem schiefwinkliger Platten*, Berlin, 1945 (available from Dr. Rongved).

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AUTHORS' CLOSURE

The authors appreciate the remarks made by Dr. Rongved and are happy to note the increasing interest in more exact analysis that will reduce the cost of rigid frame bridges.

The literature contains many solutions in finite difference form for slabs of various shapes and with various boundary conditions. However, except for certain shapes, these are not available in closed or in infinite series form. The rigid frame itself is even more complicated than a slab, for it is essentially a folded plate. A series solution of this problem has been obtained by the junior author of this paper which includes *extension as well as bending* of the plates, and satisfies the mixed boundary conditions at the knee of the frame. This solution is now being tabulated and will be presented in tabular and chart form when the calculations are completed.

The authors agree with Dr. Rongved that the only practical way to handle an analysis of this sort is to make use of solutions in chart form.

Discussion of a paper by George L. Kalousek:

Fundamental Factors in the Drying Shrinkage of Concrete Block*

By J. W. HARDING and E. C. SHUMAN

By J. W. HARDING†

Dr. Kalousek's interesting theory in which shrinkage phenomena are explained in terms of two competing mechanisms, one of which gives rise to steady shrinkage while the other, after an initial shrinkage, produces re-expansion, serves to emphasize the need for a more quantitative theory of at least one of these mechanisms if one is to disentangle the contributions of the separate mechanisms from the combined effects observed in the experimental data. For this reason I have attempted to extend slightly the existing theory in the hope that it may provide a more quantitative description of the time dependence of the capillary condensation contribution to the total shrinkage.

Let us consider a cylindrical specimen of 1 sq cm cross section and of length $2l$. It is supposed that the sides are impermeable and that evaporation takes place symmetrically from both ends into a container filled with water vapor at a known temperature and pressure and with air at atmospheric pressure.

In a normal cross section of this cylinder the distribution of pore sizes may be described by a distribution function $N(r)$, where r is the radius of a pore. Two such normal cross sections will not in general be physically congruent, although the pore distribution will be substantially the same. It seems reasonable to suppose that one can obtain a fair description of the flow processes in the specimen if one replaces the sequence of noncongruent sections by a sequence of congruent sections with the introduction of a tortuosity factor if that appears to be necessary, provided one maintains a sufficient number of cross connections between the pores to allow internal flow of water from one capillary to another.

If the specimen is in equilibrium with water vapor in the container at a relative vapor pressure less than unity, the equilibrium theory of capillary condensation tells us that all pores with radii greater than a calculable critical value will be empty, while all pores of smaller radius will be filled with water. Let us select one of the largest pores still filled with water and refer to it as pore 1. If its radius is r_1, the meniscus will be fully developed and the water

*ACI Journal, Nov. 1954, *Proc.* V. 51, p. 233. Disc. 51-10 is a part of copyrighted Journal of the American Concrete Institute, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings*, V. 51.

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in this pore will be under a tension of $2\sigma/r_1$, where σ is the surface tension of water. The menisci in all the filled pores of smaller radius will not be fully developed and will have the same radius of curvature as that of pore 1. The tension will be $2\sigma/r_1$ throughout all the filled pore space.

We now have to consider what is likely to happen if the vapor pressure in the container is suddenly reduced to correspond to the conditions in the experiments described by Dr. Kalousek. It will be sufficient to focus our attention upon two pores of different radii, pore 1 and an adjacent smaller pore of radius r_2. When evaporation begins to take place from the surface pore 1 will begin to empty and, with loss of water from pore 2, the meniscus in pore 2 will tend to become completely developed with the result that the tension in pore 2 will tend to rise to the value $2\sigma/r_2 > 2\sigma/r_1$. Provided that evaporation is not too rapid, equilibrium will be restored by the transfer of water internally by suction from pore 1 to pore 2 until the tension in pore 2 regains the equilibrium value of $2\ \sigma/r_1$. The tension will remain at $2\sigma/r_1$ and all pores of radius $<r_1$ will remain filled until pore 1 is empty, when the tension throughout the filled pore space will switch to the value $2\sigma/r_2$, where r_2 is the radius of the next largest pore, and so on.

At this stage it is appropriate to ask whether the largest pores lose water mainly by suction into smaller capillaries or by direct evaporation from the retreating meniscus. It is relevant to observe that, as the meniscus recedes into the specimen, a pressure gradient is necessary to maintain flow in the column of vapor moving from the receding meniscus to the surface of the specimen. The effective pressure drop controlling evaporation will therefore be less than if the meniscus was directly exposed to the vapor in the container, as is the case with the menisci of the smaller filled pores, and evaporation will be steadily reduced as emptying of the capillary proceeds. On the other hand a much larger number of smaller capillaries have their menisci directly exposed to the vapor in the container. It therefore seems reasonable to assume provisionally that direct evaporation plays a secondary role in the emptying of the largest capillaries and that suction of water into the smaller pores is the dominant factor, at least until an advanced stage of drying has been reached. This assumption simplifies the theoretical formulation of the problem and will be made as an interim measure. However, it is a point which requires more detailed investigation, and similar considerations arise if one considers loss of water by evaporation from side channels into larger pores already emptied. Generally speaking, it would seem that these complications are likely to become more important in the later stages of drying.

We are now in a position to set up an equation which will determine the rate at which pores empty when a specimen stabilized at one value of the relative humidity (R.H.) is suddenly subjected to evaporation loss due to the lowering of the vapor pressure in the container.

It will be assumed that the loss of water by evaporation, μ_t, at a time t is given by the expression

$$\mu_t = \alpha\,(p_t - p_c)\ \text{g cm}^{-2}\ \text{sec}^{-1} \qquad (1)$$

where

α = a numerical constant taken from data on the evaporation from flat water surfaces into a still atmosphere

p_t = value at time t of the vapor pressure in equilibrium over a concave meniscus of radius of curvature r_t

p_v = vapor pressure in the container

If r_t is the radius of the largest pore filled at time t, the relation between p_t and r_t in the equilibrium state is given by the Kelvin equation:

$$ln\ (p_t/p_0) = -\frac{2\sigma}{\rho r_t}\frac{M}{RT} \quad \ldots\ldots (2)$$

where p_0 is the equilibrium vapor pressure over a flat surface, ρ is the density of water, M is the molecular weight of water, R is the gas constant, and T is the absolute temperature.

We recall our assumption that the radius of curvature of each meniscus retains the value r_t until the pores of radius r_t are emptied. If the number of pores per sq cm with radii in the range from r to $r + dr$ is given by $N(r)dr$, the mass of water evaporating per second from each of the two exposed surfaces at time t is given by

$$m_t = \pi\ \mu_t \int_0^{r_t} r^2\ N(r)\ dr \quad \ldots\ldots (3)$$

It is, of course, not reasonable to suppose the existence of well-defined menisci down to molecular dimensions as the lower limit of integration might imply. It will be assumed that this limitation may be expressed by a suitable choice of distribution function.

Let us now consider the loss of water by suction in a small range of pore sizes, ultimately infinitesimal, from $r_{t-\epsilon}$ to r_t. The mass of water contained in pores in this range is given by

$$\pi\ l\ \rho \int_{r_t}^{r_{t-\epsilon}} r^2\ N(r)\ dr \quad \ldots\ldots (4)$$

where l is half the length of the specimen. An identical process is proceeding symmetrically from the other end.

Since the range of integration is small, we may write Eq. (4) in the form

$$\pi\ l\ \rho\ r_t^2\ N(r_t)\ (r_{t-\epsilon} - r_t)$$

which is the loss of water by suction in the time interval ϵ.

The loss of water by suction in grams per second is therefore given by

$$-\pi\ l\ \rho\ r_t^2\ N(r_t)\frac{dr_t}{dt} \quad \ldots\ldots (5)$$

which is assumed to be equal to the loss by evaporation at the surface by all pores in the range from 0 (see remarks above) to r_t, that is

$$-\ l\ \rho\ r_t^2\ N(r_t)\frac{dr_t}{dt} = \alpha\ p_0\ (e^{-\lambda/r_t} - p_v')\int_0^{r_t} r^2\ N(r)\ dr \quad \ldots\ldots (6)$$

where we have used Eq. (1) and (2) and the notation $p_v' = p_v/p_0$, $\lambda = 2\sigma\ M/\rho RT$.

With a suitable choice of distribution function $N(r)$, Eq. (6) constitutes a

differential equation determining r_t, the size of pore which is being emptied at time t. The equation is to be solved subject to the initial condition $r_t = r_0$ when $t = 0$, where r_0 is the critical pore size corresponding to the initial stabilizing value of the vapor pressure in the container. It may be noted here that the relevant temperature as far as p_t is concerned is the temperature at the evaporating surface which may, for rapid evaporation, be lower than the mean temperature of the specimen and the container.

For $N(r)$ we may, for example, choose a Gaussian distribution or there may be an advantage in using the function[1]

$$N(r) = h\,k\,\text{cosech}^2\,[k(r - R)] \quad (7)$$

which closely resembles the Gaussian distribution and can be simply integrated in closed form. This distribution is centered about $r = R$; k controls the spread of the distribution, and h may be related to the porosity or mass of evaporable water per unit volume of the specimen.

If Eq. (7) is used, the total volume of pore space per cu cm of the specimen is given by the expression

$$P = \pi\,h\,k \int_0^\infty r^2 \text{cosech}^2\,[k\,(r - R)]\,dr \quad (8)$$

If k can be chosen so that the peak of the distribution is sufficiently sharp, one may approximate to this integral by the expression

$$P \doteq \pi\,h\,k\,R^2 \int_0^\infty \text{cosech}^2\,[k(r - R)]\,dr = \pi\,h\,R^2\,(1 + \tanh\,k\,R) \quad (8')$$

In Eq. (8) the volume associated with the cross channels has been absorbed into the volume of the axial pores.

When r_t has been determined as a function of the time by integrating Eq. (6), the time dependence of several other interesting physical quantities may be found.

For example, the mass of water evaporating per second from each end of the specimen at time t, following a drop in external pressure, is given by Eq. (3), and a further integration yields the total loss of water up to a time t.

Again, the water in each filled pore is, at time t, under tension of $2\,\sigma/r_t$, which increases with the time; but, at the same time, the number of pores contributing to the tensile stress is decreasing in a manner determined by the distribution function. Tension in the pores is balanced by a compression of the solid matrix, and the compressive stress, S_t, at time t in the direction of the axis of the specimen, will be given by

$$S_t = \frac{2\,\pi\,\sigma}{r_t} \int_0^{r_t} r^2\,N(r)\,dr \quad (9)$$

where the time-dependence of r_t is, of course, governed by Eq. (6).

The percent moisture shrinkage at time t due to the capillary condensation mechanism will then be given by

$$100\,\epsilon_t = \frac{200\,\pi\,\sigma}{E\,r_t} \int_0^{r_t} r^2\,N(r)\,dr \quad (10)$$

where ϵ_t is the compressive strain and E is the elastic modulus for the solid matrix.

Moisture shrinkage will be a maximum when

$$d\,\epsilon_t/dt = 0 \qquad (11)$$

Differentiating Eq. (10) and equating the result to zero, we find

$$r_t^3\,N(r_t) = \int_0^{r_t} r^2\,N(r)\,dr \qquad (12)$$

Eq. (12) determines the size of pore which is being emptied when shrinkage (that is, that part of it which is due to the capillary condensation mechanism) reaches a maximum and, through Eq. (6), the time at which this takes place.

By comparing the predictions of Eq. (3), (6), (10), and (12) with experimental data one may hope to fix limits to the arbitrary parameters h, k, and R in a distribution function such as Eq. (7).

To get an idea of the order of magnitude of the time taken to reach equilibrium, let us consider a specimen in which the pore distribution is given by Eq. (7). Let us suppose that the distribution is centered about $R = 10^{-6}$ cm, and that k is so chosen that $N(r)$ falls to half its maximum value when $r = 2 \times 10^{-6}$ cm, which gives $kR = 0.88$. $R = 10^{-6}$ cm is the critical radius for a specimen stabilized at 0.90 R.H., while $r_0 = 2 \times 10^{-6}$ cm is the critical radius for stabilization at 0.95 R.H. We now estimate the time taken by a specimen stabilized at 0.95 R.H. to reach equilibrium when the external relative vapor pressure is reduced to 0.90.

An estimate of α in Eq. (1) may be obtained from Rohwer's empirical formula.[2] When Rohwer's coefficient is converted to the units used in this paper one finds $\alpha = 3.50 \times 10^{-10}$ cm^{-1} sec.

If, in Eq. (6), we use these values together with $l = 0.5$ cm, $\rho = 1$, $p_0 = 1.69 \times 10^4$ dynes-cm^{-2} (at 15 C), $\lambda = 1.1 \times 10^{-7}$ cm, we find

$$\left(\frac{dr_t}{dt}\right)_{r_t = r_0} = -\,9.5\,(0.947 - p_v')\times 10^{-12}\ \text{cm sec}^{-1} \qquad (13)$$

where we have approximated to the integral in Eq. (6) by writing

$$\int_0^{r_t} r^2\,N(r)\,dr \doteq R^2 \int_0^{r_t} N(r)\,dr \qquad (14)$$

The problem amounts to finding the time required for r_t to change from an initial value of 2×10^{-6} cm (0.95 R.H.) to a final equilibrium value of 10^{-6} cm (0.90 R.H. $= p_v'$). As equilibrium is approached, dr_t/dt tends to zero, so that a rough estimate of dr_t/dt over the range we are considering will be given by half the value calculated in Eq. (13).

With this assumption and with $p_v' = 0.90$, we find from Eq. (13)

$$\delta r_t = -\,10^{-6} = -\,4.75 \times 0.047 \times 10^{-12}\,\delta t$$

which gives for the time δt taken to reach equilibrium a rough estimate of $\delta t = 50$ days.

The moisture shrinkage, calculated from Eq. (10), for the same specimen in the initial equilibrium state with critical radius $r_0 = 2 \times 10^{-6}$ cm, assuming P (Eq. 8′) $= \frac{1}{3}$ and $E = 10^{11}$ dynes-cm^{-2}, is approximately 0.02 percent.

Finally, attention is recalled to the fact that the simplicity of the formulas given in this note depends on the basic assumption that evaporation must

not be so rapid that the suction mechanism fails to maintain the radii of curvature of all menisci at the same value as that of the largest pores which are being emptied.

If there is a sudden large drop in the relative vapor pressure maintained in the container, the inertia of the water in the pores will become an important factor, particularly in the smaller pores. The menisci of the smaller pores at least will tend to become fully developed, and evaporation will be partly checked. This effect may be enhanced by a drop in temperature at the surface of the menisci due to rapid evaporation. There will be a corresponding rise in the compressive stress and shrinkage. After this first "shock" is over, in a time interval which the present elementary theory does not allow us to calculate, conditions will tend to return to normal so that the suction mechanism is once more effective in maintaining a quasi-stationary state in which loss of water from the smaller pores is compensated by flow from the largest pores which are being emptied. There may be a re-expansion; but not to the original value, since some of the larger pores will have emptied during the interval. The model we have used thus suggests that there may be steps or "kinks" in the curve of shrinkage or moisture content plotted against time, if successive drops in external vapor pressure are large enough to overload the suction-compensation mechanism.

The writer wishes to express his appreciation of several stimulating discussions with Dr. F. M. Lea on this and related problems of water adsorption.

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By E. C. SHUMAN*

The data and discussion of dimensional changes of hydrous calcium silicate materials presented by Dr. Kalousek contributes markedly to an ultimate understanding of so-called drying shrinkage. While the data the writer is presenting are not sufficient to extend greatly the knowledge on the subject, it does corroborate some of the author's observations and adds to the scope of information available.

It should be explained that these data were obtained in 1946 as a part of investigations conducted in an East Coast plant and, therefore, not available in detail to the author until the subject paper was published, and its information recalled to the writer data in the files that have since been shown the author as a matter of corroboration of some of the behaviors he cited.

The change of length measurements were made on hydrous calcium silicate materials, probably of the monocalcium silicate variety, having nominal densities of 11 and of 20 lb per cu ft and containing asbestos fibers. The specimens were 1 x 1 x 10-in. and 2 x 2 x 10-in. bars cut from larger slabs.

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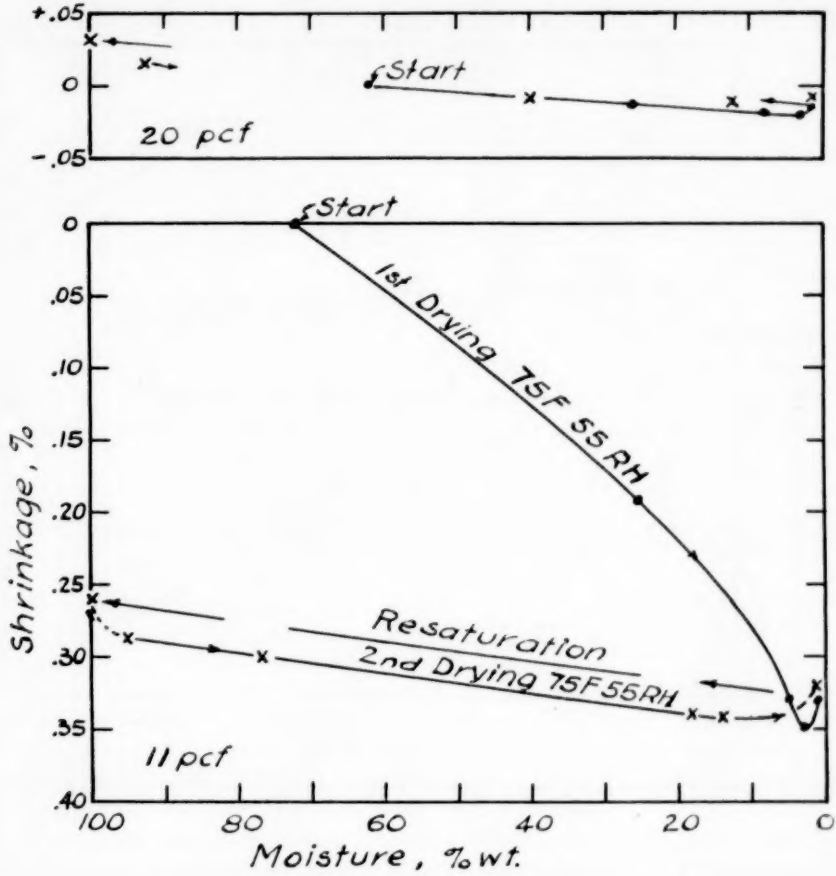

Fig. A—Relation between moisture change and length change for 11 and 20 lb per cu ft hydrous calcium silicates in 75 F, 55 R. H., air

The plotted data in Fig. A, B, and C show that specimens were not fully saturated upon removal from the autoclaves in which they were made, which is due to "flash" of a part of the intersticial water with pressure release.

Since the dry weight depends upon the temperature of drying and time, the "zero" percentage of moisture is not precise and, therefore, the plotted values at zero are not literally correct; also, materials having porous structures with internal surface areas of 20 to 150 sq m per g probably can not be completely saturated by immersion and therefore the indication of 100 percent moisture is also an approximation. Neither of these factors in themselves are significant to the discussions. It should be noted that the data are presented on the basis of "saturation" as 100 percent moisture, rather than on the basis of dry weight of materials as is often used.

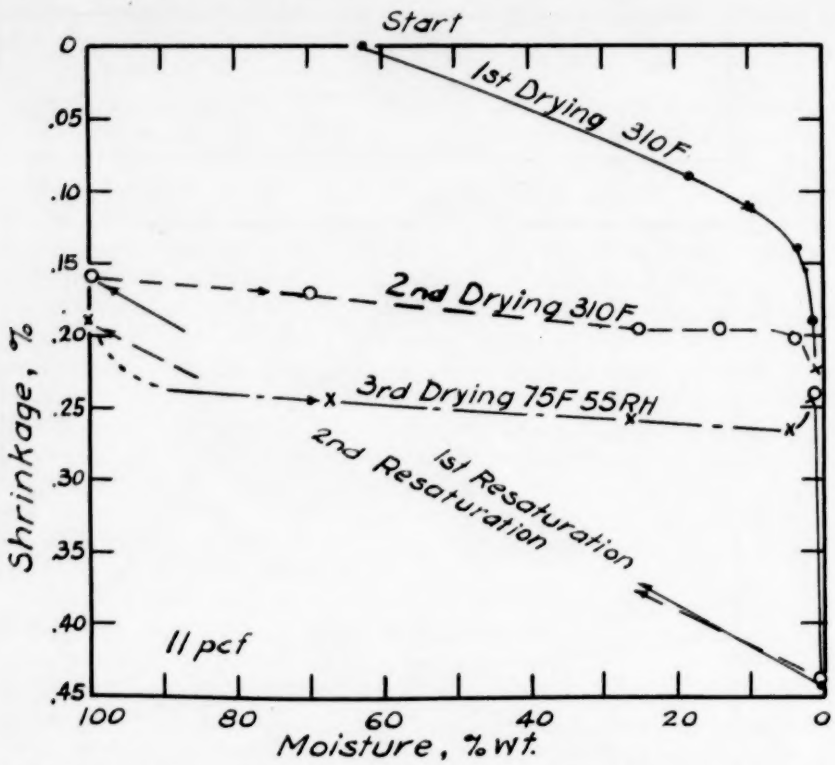

Fig. B—Relation between moisture content and length for 20 lb per cu ft hydrous calcium silicate subjected to 310 F and 75 F, 55 R. H., air

It should be noted that length changes frequently take place more abruptly near both the "dry "and the "saturated" states.

The data show that upon drying hydrous calcium silicates there is a definite expansion upon continued drying near the "dry" state as the author has shown.

Since "first" drying in either a 310 F oven or an air-conditioned room produces changes that are not fully reversible there is a change in structure during first drying. This first change is such that one should consider first drying as a part of the over-all process of making dry hydrous calcium silicate products for structural uses. In general, subsequent wettings cause less length change effects if the first drying produces as much shrinkage as possible. If this in itself causes cracks, then corrective means for avoiding cracks in manufacturing will minimize cracks in service.

Fig. A shows first drying with subsequent immersion in water and redrying of both 11 and 20 lb per cu ft hydrous calcium silicates at 75 F, 55 R.H., and should be compared with Fig. B and C which show similar length changes due to drying at 310 F with the third or fourth drying at 75 F, 55 R.H. The

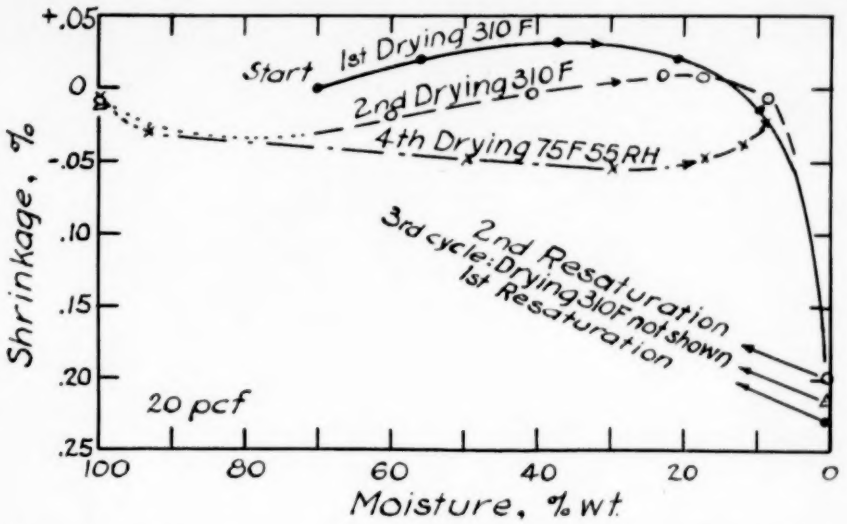

Fig. C—Relation between moisture content and length for 11 lb per cu ft hydrous calcium silicate subjected to 310 F and 75 F, 55 R. H., air

random dotted curves near 100 percent moisture indicate that insufficient data were obtained to establish the curve in this region. Except for the behavior at the dry and wet ends of the graphs, there is a remarkable parallelism in change in length with change in moisture for repeated cycles after the first drying for both densities for both drying conditions except the 310 F drying in Fig. B. In Fig. B, the third cycle (immersion and redrying) was omitted because the points lay so close to the second drying that they would be confusing.

These behaviors conform to the author's statement, p. 241, that near the dry state strain is released when the capillaries are emptied of liquid water.

The abrupt shrinkage below 5 percent moisture shown in Fig. 6 was not observed. However, other tests verify that the opposing stresses to which the author refers do vary. These seem to the writer to be due to variation in completeness of the hydrous calcium silicate reaction, including inversions that can occur, the elastic properties, internal structure including that due to apparent density, and the effect of air taking the place of the water leaving as vapor.

The writer acknowledges the careful work in 1946 of William Y. Inouye in obtaining the data presented.

Discussion of a paper by Gordon W. Greene:

Test Hammer Provides New Method of Evaluating Hardened Concrete*

By ARTHUR R. ANDERSON, DELMAR L. BLOEM, E. L. HOWARD, PAUL KLIEGER, HAROLD SCHLINTZ, and AUTHOR

By ARTHUR R. ANDERSON†

Mr. Greene's paper is a valuable contribution to the literature on nondestructive testing of concrete. He described the Schmidt concrete test hammer, and presented factual data on the Model I type hammer. The writer has had considerable experience with both types of test hammer, and submits data on the Model II Schmidt concrete test hammer.

Fig. A is a plot of several hundred calibration tests showing the relationship between cube compressive strength of concrete and Model II hammer rebound. These tests were made at the Swiss Federal Testing Laboratory on specimens ranging from 7 to 90 days old. The specimens were from both laboratory and field construction projects, and represent a wide range of concrete mixes.

Fig. B is a plot of tests carried out by the writer, with the Model II hammer, mainly on high-strength concretes in two age groups. Note that the younger concretes agree substantially with the Swiss data for relationship between compressive strength and hammer rebound number, while the older concretes show a higher rebound number for a given compressive strength. Schmidt has recognized the fact that concrete continues to harden with age at a rate higher than the compressive strength gain, and for this reason states that the calibration curve furnished with the instrument is applicable to normal types of concrete in the age range of 7 to 90 days, which covers the important age bracket during construction.

Fig. C is the result of tests with the Model II hammer on lightweight concretes made from expanded shale, the concrete having a density of 115 lb per cu ft. Note that the lightweight concrete curve falls below the curves for concretes made from hard rock aggregates.

The data presented in Fig. 4 show excellent correlation between test hammer rebound and cylinder compressive strength. In general, the correlation between test hammer rebound and compressive strength is more difficult to obtain with cylinders than it is with cube specimens. As Mr. Greene

*ACI JOURNAL, Nov. 1954, *Proc.* V. 51, p. 249. Disc. 51-11 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

†Concrete Engineering Co., Tacoma, Wash.

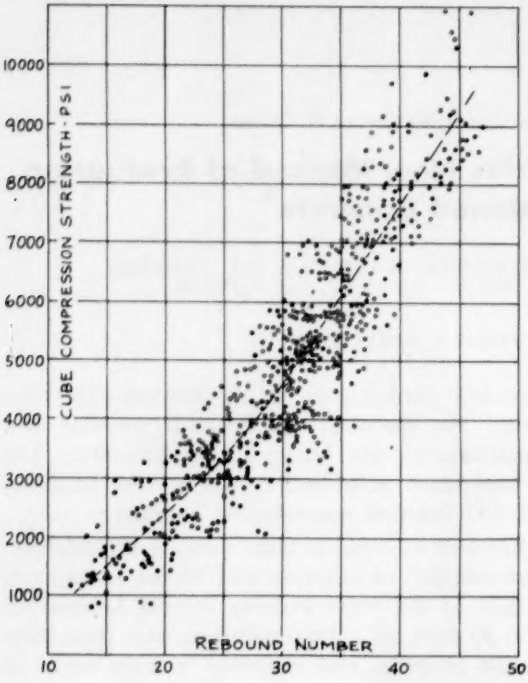

Fig. A—Relationship between cube compressive strength and Model II test hammer rebound number, determined by Swiss Federal Testing Laboratory, for [illegible] hard aggregate concretes

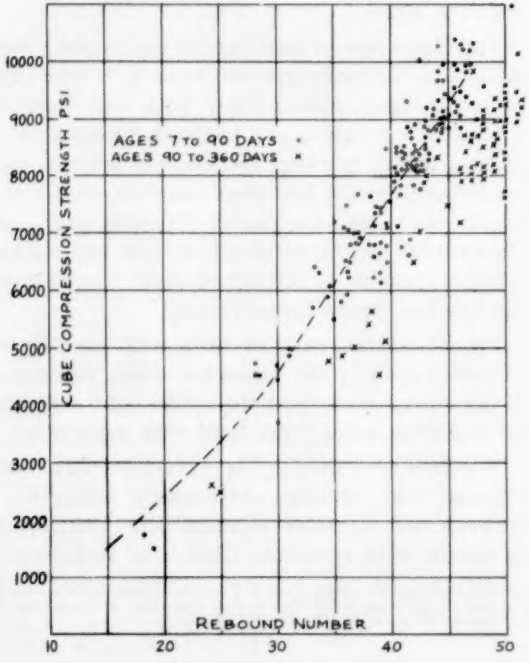

Fig. B—Relationship between cube compressive strength and Model II test hammer rebound number, from writer's tests, for hard aggregate concretes

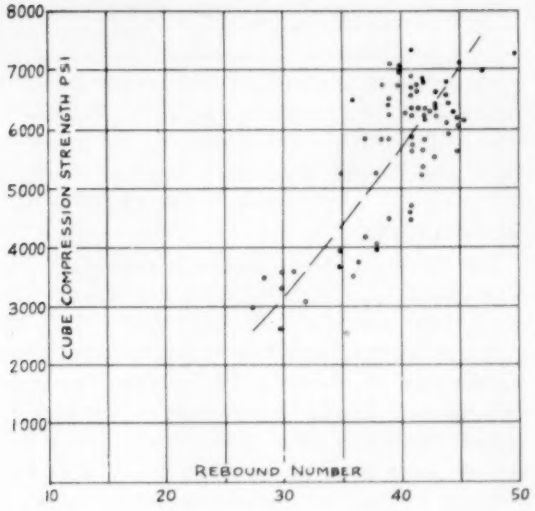

Fig. C—Relationship between cube compressive strength and Model II test hammer rebound number for Haydite concretes

mentions in his paper, capping materials and laboratory equipment and techniques are possible sources of error. The writer, testing a number of cylinders from a given batch of concrete has observed considerable dispersion of compressive strength. By means of SR-4 gages attached to the sides of the cylinders it was found that eccentricity of loading in varying amounts was always present. The lower apparent compressive strengths could be attributed to higher eccentricities.

In obtaining test hammer rebound readings against a test specimen, whether it be a cylinder or a cube, the concrete must be rigidly supported, as shown in Fig. D; otherwise part of the hammer impact energy will be lost in displacing the mass of the test specimen.

The concrete test hammer offers the field engineer a convenient means of nondestructive testing of concrete in place. Its accuracy approaches that of carefully prepared cylinder tests, and because of its simplicity and ease of operation, literally hundreds of tests can be made in a short time.

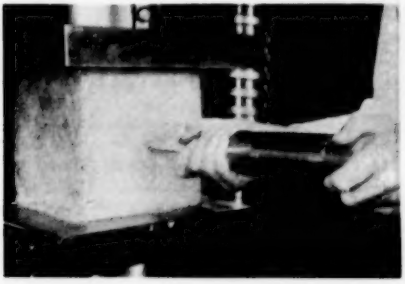

Fig. D—Concrete specimen must be rigidly supported during calibration tests

By DELMAR L. BLOEM*

As a supplementary means of evaluating and checking concrete strength characteristics, the Swiss test hammer merits thorough investigation. It is doubtful, however, that it can be relied upon to give a quantitative measure of strength. Data developed in the research laboratory of the National Sand and Gravel Assn. and National Ready Mixed Concrete Assn. at the University of Maryland are less encouraging than those presented in Mr. Greene's paper.

Fig. E shows the relationship between compressive strength and rebound reading for a portion of the cylinders tested during an investigation of concrete truck mixers in the summer of 1952. The concretes represent a range in water-cement ratio but were all made with the same aggregates and the same brand of cement. The rebound readings were made with a Model I test hammer on the lower ends of the test cylinders which had been cast against machined steel base plates. Each hammer value is the average of five or more readings on a given cylinder and is plotted against the compressive strength of that particular cylinder.

Statistically, the relationship between strength and hammer reading is good as evidenced by a correlation coefficient for the least-squares regression line of 0.875. Nevertheless, it is evident from the curve that a given hammer reading may represent a range in strength of as much as 2000 psi. For example, hammer readings in the range of about 30 to 35 in this case correspond to strengths from 2000 to over 4000 psi.

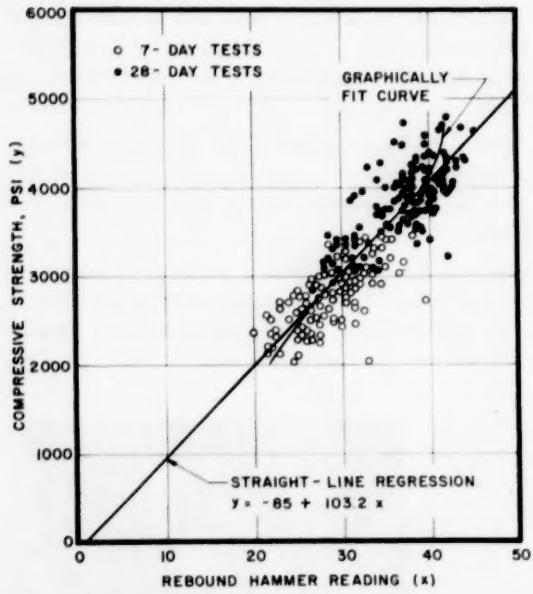

Fig. E—Relationship of compressive strength of concrete to rebound hammer test reading (Series 149)

Fig. F and G show data from a second investigation involving air-entrained and non-air-entrained concretes of various cement factors made with two different coarse aggregates. The same cement was used throughout. Hammer readings again were made with the Model I test hammer, this time on the formed faces of the broken halves of 6 x 6-in. beams used for flexural strength

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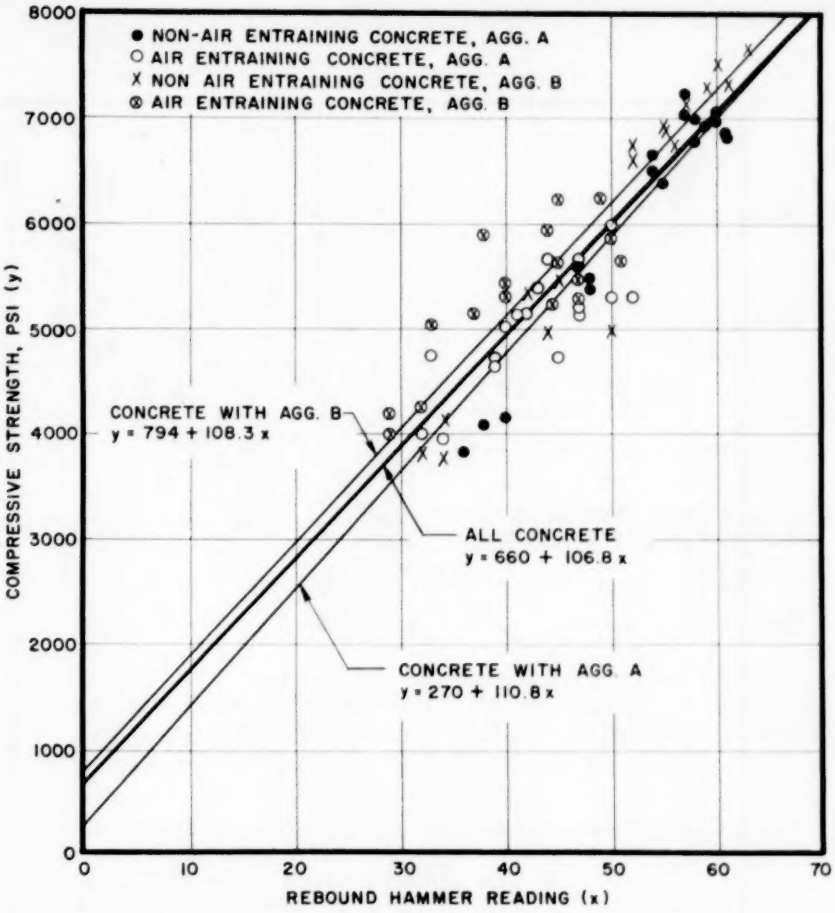

Fig. F—Relationship of compressive strength of concrete to rebound hammer test reading (Series 155)

tests. Each point on these curves represents the average of 12 hammer readings. They are plotted against the compressive strength of a 6 x 12-in. cylinder from the same batch in Fig. F and against the modulus of rupture of the beams in Fig. G.

Fig. F agrees with Fig. E in that there is a good correlation between compressive strength and hammer reading but, again, the spread in strength for a given reading is quite large. Calibration curves for the two different types of coarse aggregate used were not significantly different nor does there appear to be a consistent difference between air-entrained and non-air-entrained concrete.

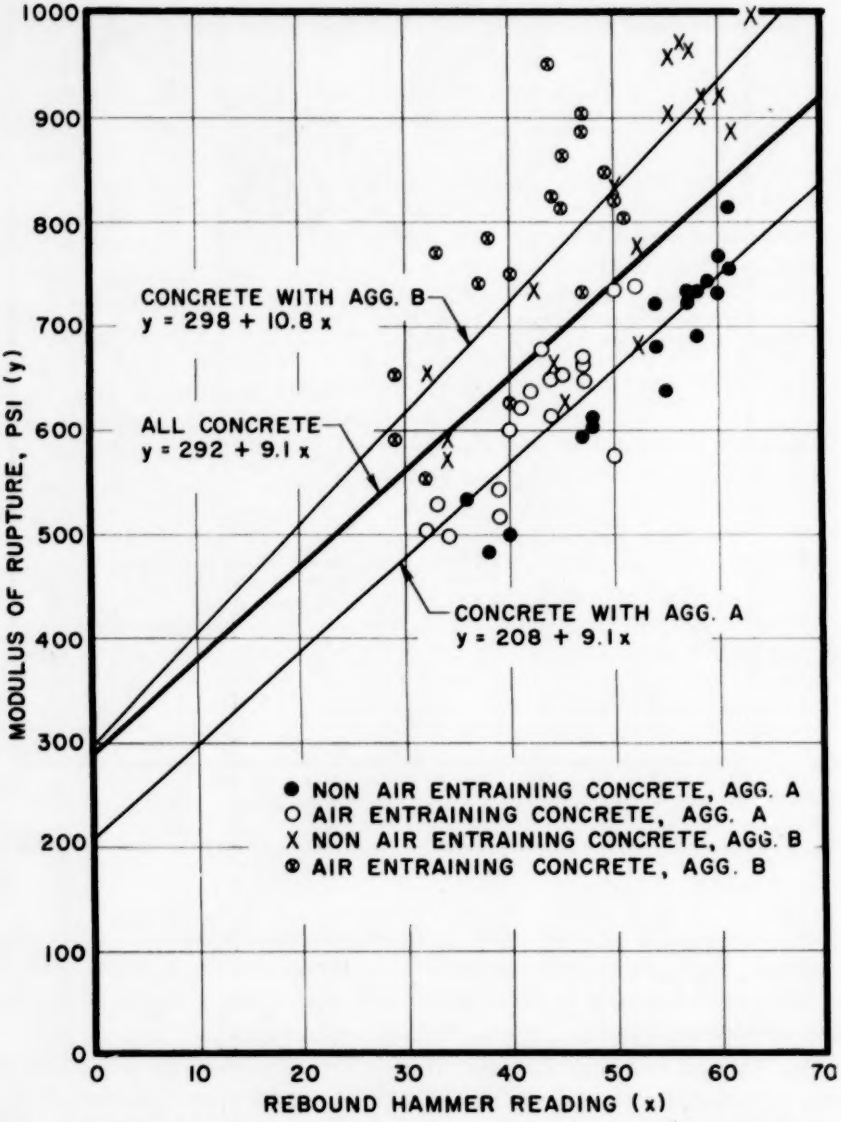

Fig. G—Relationship of flexural strength of concrete to rebound hammer test reading (Series 155)

It should be noted that the calibration curves in Fig. F are markedly different from that in Fig. E. It is not known whether this is due to the difference in mass of the specimens on which hammer readings were taken or to failure of the test hammer to maintain its calibration. The former would seem more probable.

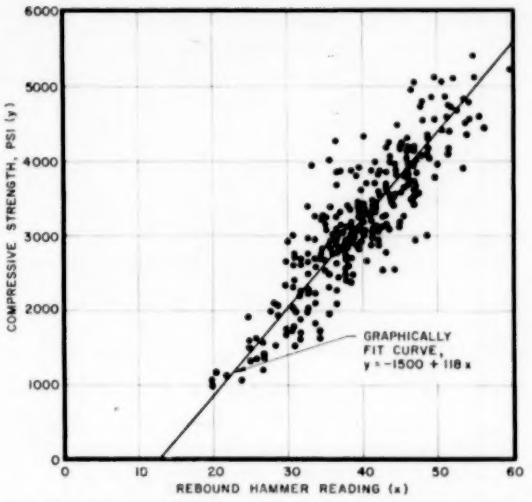

Fig. H—Relationship of compressive strength of concrete to rebound hammer test reading (Central Pre-Mix Concrete Co.)

Fig. G confirms Mr. Greene's conclusion that hammer readings are less reliable for estimating flexural strength than compressive strength. There is a considerable difference between the two types of aggregate and, even for a single aggregate, the spread in flexural strength is large for a given reading.

Fig. H shows data from field tests of ready-mixed concrete submitted by J. Gordon Fenton, engineer for Central Pre-Mix Concrete Co., Spokane, Wash. The readings were made with a Model I test hammer on the lower end of compression test cylinders. Here again, for the particular materials and conditions of test, a good relationship of compressive strength to hammer reading is evidenced. The spread in strength of up to 2000 psi for a given hammer reading shown here is comparable with that in Fig. E and F.

The relationship between compressive strength and hammer reading in Fig. H differs considerably from that in either Fig. E or F and, as pointed out before, these in turn differ from one another. Possible reasons for differences in these might include the nature of the aggregates and, in the case of Fig. F, the mass of the concrete upon which the rebound readings were taken. In any event, data in the three figures taken together show an extremely wide range in compressive strength for a given hammer reading. For example, a rebound reading of 30 might indicate a compressive strength as low as 1500 psi or as high as 5000 psi.

Recently the laboratory acquired a Model II test hammer which was compared with the Model I hammer in readings taken on broken flexural strength specimens ranging in cross section from 3 x 4 in. to 10 x 10 in. Non-air-entrained concretes with 5½ sacks of cement per cu yd and 2- to 3-in. slump were made using gravels of 1-in. and 2-in. maximum size from the same source. The tests were made to study level and reproducibility of

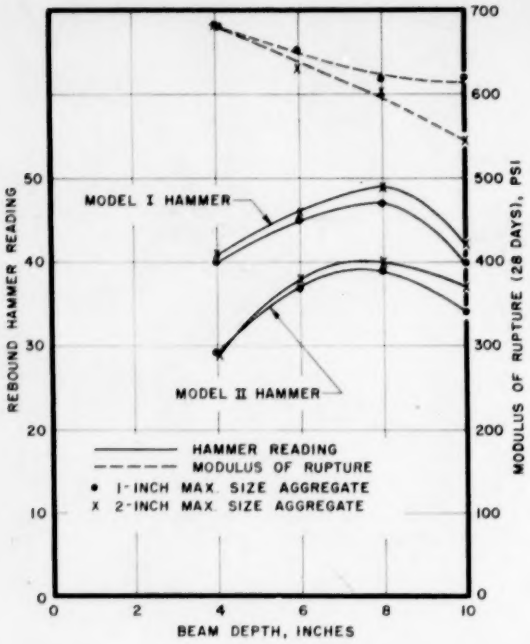

Fig. I—Effect of size of specimen on rebound reading and flexural strength of concrete (Series 155C)

flexural strength for different size specimens using third-point loading. No compression tests were made.

Fig. I shows the relationship of specimen size to hammer readings and to measured flexural strength. Each point for hammer readings represents the average for ten specimens, each tested 24 times, for a total of 240 readings. Flexural strength results represent the average for ten specimens from individual batches of concrete.

The Model II hammer gave readings averaging about 20 percent lower than the Model I hammer regardless of size of specimen. The 1-in. maximum size aggregate produced slightly higher flexural strengths but lower test hammer readings than the 2-in. aggregate. Measured flexural strengths were reduced as specimen size increased but, for both models of hammer, readings increased up to the 8-in. size but were less for the 10-in. beams. It appears, therefore, that the inertia of the specimens was not the only factor affecting hammer readings even though all specimens were molded from the same batch and were, therefore, presumably of identical concrete. The only reasonable explanation developed so far for the low hammer readings on 10-in. beams was that they were molded in wood forms while all other sizes were made in steel molds. Thus it appears that surface finish can be included as one of the extraneous factors which affect the test hammer results.

These data strongly suggest the need for reservation in accepting readings of the Swiss test hammer as a quantitative measure of concrete strength.

It appears that a number of factors in addition to strength affect the magnitude of the hammer readings. Examples are size of specimen, surface finish, and probably the nature of materials used in the concrete. In the opinion of this writer, results of Swiss hammer tests must be considered only as a rough indication of strength level unless a great deal of background information has been developed for a particular set of materials and test conditions.

By E. L. HOWARD*

In discussion with engineers about when to strip the forms from concrete, the old-timer's best argument was a ballpeen hammer. A smart blow of the hammer on the concrete indicated the strength. Some fellows covered the spot to be tested by a bit of carbon paper; the size of the impression left indicated strength. It is not surprising that someone at last found a scientific adaptation of the old-timer's hammer test.

We have used the Schmidt testing hammer since 1951. Both the Model I (basis for the data in Mr. Greene's paper) and the Model II are effective instruments to determine compressive strengths of concrete. Much work has been done in the laboratory with the Model II. Just before capping, the 6 x 12-in. cylinders were tested with the hammer. These cylinders were then broken in the standard manner to obtain compressive strength. Data from these tests are shown in Fig. J. The "x's" are Haydite concrete (Haydite mix has ½ normal sand).

The quality of concrete in a building is usually evaluated from samples taken from the concrete as delivered. The samples, usually 6 x 12-in. cylinders, are taken to a laboratory for storage and testing. They sometimes lead an exciting life from casting to testing. What with falling lumber, heavy trucks, wandering workman, and even the weather, the cylinders often get badly scarred or damaged before they reach the curing room. Casting of the specimen may not have yielded a concrete sample representative of the material in the job. Testing may not υ ing out the whole truth about the cylinder's strength. A compression test that is lower than specification may not, therefore, prove that the job concrete is below specifications. How can we know if the job concrete is sound? This has long been an expensive question. It is here that the hammer can be of help. The following examples show on the job uses of the Schmidt hammer.

School job—Compression tests of specimens purported to represent a foundation placement well below specifications. Hammer tests taken on the whole length of foundation indicated no poor concrete. The cores that were cut and tested confirmed the findings of the test hammer.

Apartment house job—Cylinders made from one wall placement yielded strengths much lower than the design. The exact location of this tested load was not known. Hammer tests of the whole wall area located the section of low-strength concrete.

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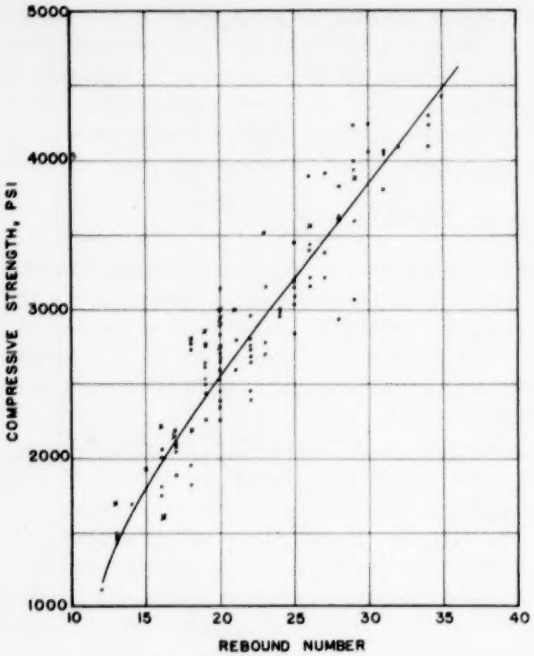

Fig. J—Comparison of rebound reading and compressive strength of ordinary and lightweight concrete

Warehouse job—Test cylinders from this job varied greatly from very low to just above specification. The hammer tests indicated uniform strengths throughout the entire slab. It was later determined that rough handling in transit damaged the test cylinders.

Hospital job—Concrete in the piling was in doubt. The location made coring a difficult task. The rebound hammer easily determined the comparative quality of concrete in the piles in the cluster in question.

The rate of strength gain of concrete can be determined easily with the test hammer. A concrete pipe manufacturer used the hammer to set the curing time of some pipe cured under steam tents. A tunnel contractor determined the time for slipping forms by testing the walls and arch with the hammer. A contractor building warehouses with tilt-up panels takes rebound tests before lifting. A job in Korea is being controlled with the hammer to save the expense of a compression machine. Occasional cylinders are flown to Tokyo to be broken as check tests for the hammer results. Destruction of buildings by fire leave some doubt as to strengths of concrete columns, walls, and foundations. In these and similar cases the test hammer is an invaluable tool.

By PAUL KLIEGER*

Mr. Greene is to be commended for presenting the extensive amount of data obtained with the concrete test hammer. The over-all impression given the reader, however, is that the data obtained with this instrument are reliable measures of concrete strength. For example, Mr. Greene states that the ". . . test hammer utilizes the principle that the rebound of a steel hammer is proportional to the compressive strength of the concrete." He further states that the ". . . test hammer provides a quick, inexpensive method of checking the quality and strength of hardened concrete." The

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first statement concerning principle is not supported by theoretical considerations, while the second statement results from insufficient data with regard to the possible effect of various differences among concretes on the correlations obtained.

There appears to be no theoretical relationship between the strength of a material and the rebound of a steel hammer striking the material. However, empirical relationships derived for a particular set of limiting conditions have proved useful and therein lies the greatest value of an instrument such as the concrete test hammer.

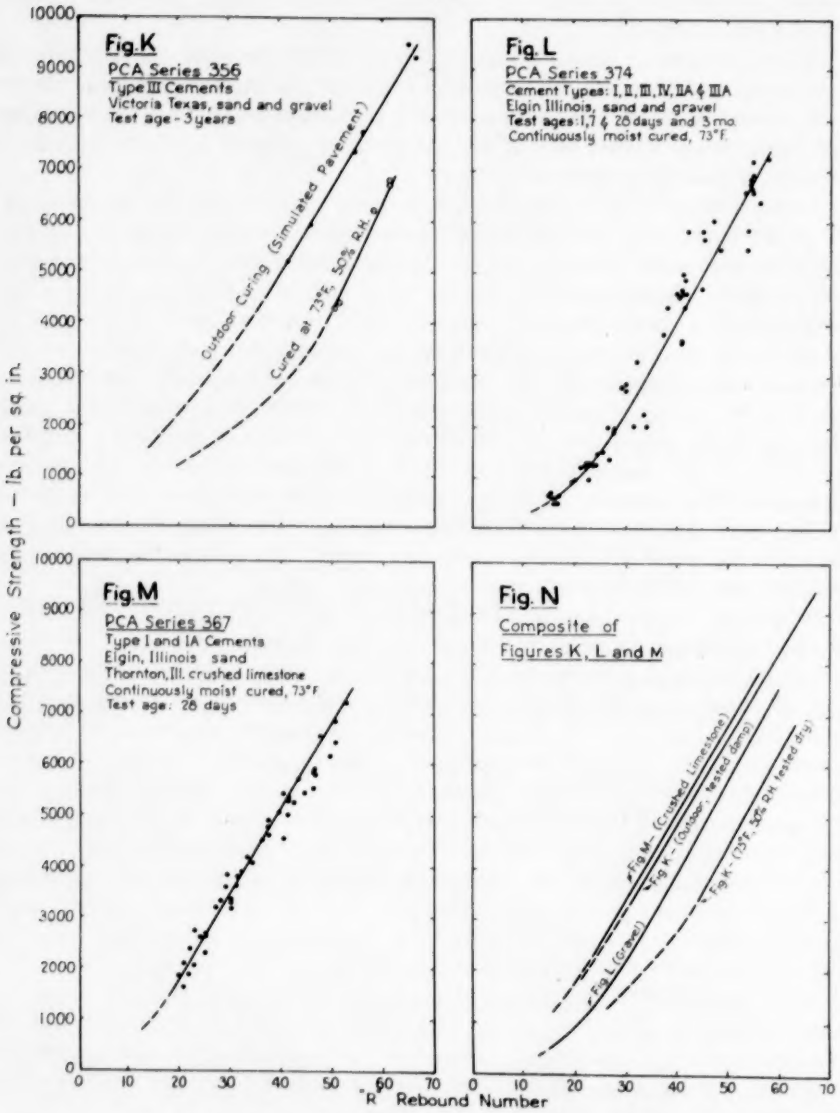
There is some question as to what property is measured by the hammer. It might be well to consider what happens to the energy imparted by the hammer during the impact. If the material being tested is large and perfectly elastic, all of the energy imparted is returned to the hammer. Concrete is not a perfectly elastic material and therefore part of the energy imparted by the hammer is dissipated, the amount being dependent on the characteristic viscosity of the material. This characteristic viscosity, or internal friction, is probably a function of the elastic properties of the paste and aggregate and is therefore likely to be different for different concretes. In addition, the hammer blow produces a permanent dent in the surface during impact. Part of the energy imparted by the hammer is lost by virtue of the work required to produce this crushing. Therefore, the energy loss involved is composed of two parts: the energy dissipated due to internal friction and the energy lost due to localized crushing.

If the total energy loss is almost wholly due to localized crushing, under proper conditions of testing a good correlation with compressive strength might be expected. However, if the larger part of the energy loss is due to the effect of viscosity, or internal friction, the correlation might be poor unless the conditions affecting the viscosity remained constant. Concretes made with different types of aggregates might be expected to have different characteristic viscosities. In addition, changes in the moisture content of the concrete might not only result in changes in viscosity of the paste phase and therefore of the concrete, but free water present in the voids would absorb part of the energy on impact. It should be recognized that the effect of factors influencing only the surface region of the concrete specimen, such as carbonation, might be reflected to a larger degree in rebound hammer test results than in conventional strength tests of the gross specimen.

These questions can be answered most conveniently by using the concrete test hammer on concretes made with different types of aggregate and at various degrees of saturation. Data have been obtained in this laboratory utilizing specimens of different concretes, prepared for other studies, that were being tested for strength at this time.

DISCUSSION OF RESULTS

All of the specimens tested with the concrete test hammer were 6 x 6 x 30-in. concrete beams. These beams were tested in flexure (third-point loading



Relationship between rebound number and compressive strength

on an 18-in. span, two breaks per beam) and the beam ends tested in compression as 6-in. modified cubes. The remaining central portion was used for the hammer test. The rebound number for a particular specimen was the average of determinations made on the three molded surfaces of the specimen: the bottom and two sides. Some tests were made on a portion of the top surface ground smooth with the carborundum stone furnished. These rebound numbers ranged from 5 to 15 percent lower than those obtained on the molded surface and the reproducibility was poor.

Fig. K shows the relationship between rebound number and compressive strength of 3-year old specimens made with Type III cements and a sand and gravel from Texas. Companion specimens were stored under two different conditions following a 7-day preliminary moist room storage: one, outdoors on a soil subgrade simulating a pavement exposure, and the other in the laboratory at 73 F and 50 percent relative humidity. The curve for the specimens cured outdoors and tested in their relatively damp condition is in good agreement with the calibration curve furnished by the manufacturer of the test hammer. However, the curve for the concretes stored in air and tested dry is considerably lower than for the moist storage and falls well below the calibration curve, indicating the effect of moisture condition of the concrete on the hammer rebound. For these two curing conditions, at equal compressive strengths, the rebound numbers obtained for the concretes stored in the air of the laboratory are generally 10 to 12 units higher than those for the concretes stored outdoors. This difference in rebound numbers represents approximately 2000 psi compressive strength.

Fig. L shows the data for specimens made with a number of the PCA Long-Time Study cements and one sand and gravel from Illinois. The specimens were continuously moist cured and tested at ages ranging from 1 day to 3 months. The curve falls slightly below the lower standard deviation line of the calibration curve. The individual data show a considerable amount of scatter from the average curve drawn through the points. The scatter does not appear to be influenced by the cement type. The cement types represented were I, II, IIA, III, IIIA, and IV.

Fig. M represents data obtained with the same fine aggregate as in Fig. L (Elgin, Ill.) and a crushed limestone coarse aggregate from Thornton, Ill. The maximum size of coarse aggregate in the various mixes used ranged from $\frac{3}{8}$ to $1\frac{1}{2}$ in. The cements were Type I and IA. Specimens were cured continuously moist and tested at 28 days. Less scatter of data is evident. The concretes in Fig. L and Fig. M are essentially similar except for the difference in type of coarse aggregate. This provides us with a fairly good example of the effect of aggregate type on the data obtained with the concrete test hammer. For equal compressive strengths, the concretes made with the crushed limestone coarse aggregate show rebound numbers approximately 7 units lower than those for the concretes made with the gravel coarse aggregate. This difference in rebound numbers represents approximately 1000 psi compressive strength.

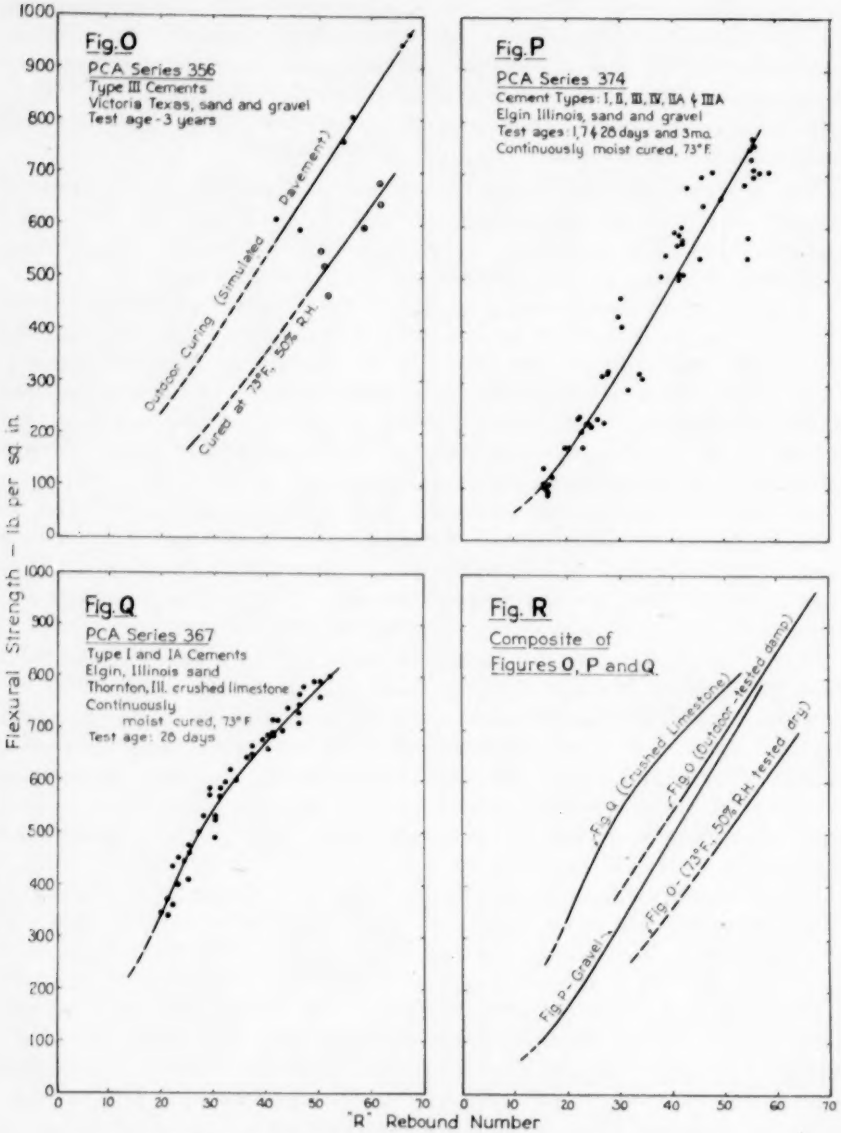


Fig. O
PCA Series 356
Type III Cements
Victoria Texas, sand and gravel
Test age - 3 years
Outdoor Curing (Simulated Pavement)
Cured at 73°F, 50% R.H.
Fig. P
PCA Series 374
Cement Types: I, II, III, IV, IIA & IIIA
Elgin Illinois, sand and gravel
Test ages: 1, 7 & 28 days and 3 mo.
Continuously moist cured, 73°F.
Fig. Q
PCA Series 367
Type I and IA Cements
Elgin, Illinois sand
Thornton, Ill. crushed limestone
Continuously moist cured, 73°F
Test age: 28 days
Fig. R
Composite of Figures O, P and Q
Fig. Q (Crushed Limestone)
Fig. O (Outdoor - tested damp)
Fig. O - (73°F, 50% R.H. tested dry)
Fig. P - Gravel
Flexural Strength - lb. per sq. in.
"R" Rebound Number

Relationship between rebound number and flexural strength

Fig. N is a composite of Fig. K, L, and M presenting the average relationships shown in each figure. This composite illustrates the shifting resulting from change in moisture condition and change in aggregate type.

Fig. O, P, Q, and R show the relationships between flexural strengths of these same concretes and hammer rebound number. The relationships are similar to those obtained for compressive strength, the individual data showing somewhat more scatter. The effects of moisture condition and aggregate type are similar to those found for the compressive strength-rebound number relationships.

SUMMARY

The data presented above indicate that the empirical relationship obtained between rebound number and concrete strength is affected by: (1) moisture content of concrete at time of test and (2) type of aggregate.

The effect of moisture content and type of aggregate limits the usefulness of the concrete test hammer. However, a "job curve" of rebound number versus concrete strength determined from test specimens made with the materials to be used in the structure and cured under conditions similar to those for the structure would make the instrument of some value on the job site. Indiscriminate use without considering the above two factors might result in erroneous interpretation of results.

By HAROLD SCHLINTZ*

Extensive work was done by myself at Twining Laboratories, Fresno, Calif., on correlating Schmidt test hammer results with those of the compression machine. In general, Mr. Greene's conclusions are valid.

I have used the test hammer to predict the compressive strength of concrete which was later cored to prove the strength requirements, and test hammer results were within the probability limits of the concrete itself. Age of the concrete varied from 77 to 35 days. Results are tabulated in Table A.

Cores No. 3 and 6 were taken from an area not checked with the test hammer, but out of the same day's placement. The concrete was designed for 2000 psi in 28 days and one 6 x 12-in. test specimen was made for each day's place-

TABLE A—RESULTS OF TESTS ON CONCRETE FOUNDATION SHOWING CORRELATION BETWEEN TEST HAMMER AVERAGE COMPRESSIVE STRENGTH AND COMPRESSIVE STRENGTH OF 6-IN. CUT CORE

Core No.	1 and 2	3	4	5	6
Age—Test hammer, days	63	60	48	37	35
Age—Core, days	77	74	62	51	49
Average test hammer value, psi	2400	3000	2800	2400	3600
Core compressive strength, psi	3625	2220	3160	2730	2840
28-day compressive strength, psi	1310	1730	2445	1880	2970

*Civil Engineer, Fresno, Calif.

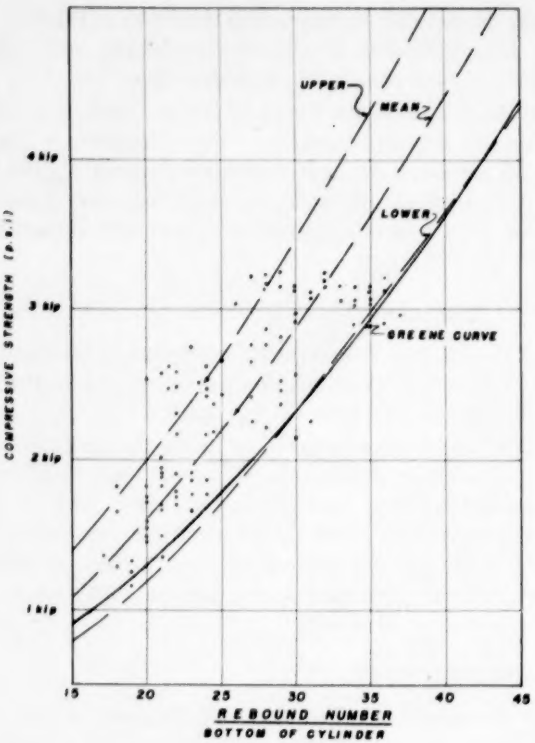


Fig. S—Rebound numbers versus compressive strength—bottom of cylinder

ment. The test hammer values were no more erratic than the 28-day cylinder tests or the core tests. A Model I hammer was used and Mr. Greene's curve was used to obtain the corresponding compressive strength. Sixteen hammer readings were taken for each placement, and the three highest and the three lowest readings were disregarded. The average of the remaining ten readings was used to determine the compressive strength.

In another experiment at Twining Laboratories, 106 concrete cylinders were cast from the same mix and tested at 7, 14, and 28 days. Prior to breaking the cylinders in the compression machine, a Model I hammer was used to obtain four readings on the top and four on the bottom of the cylinder. In taking the readings, the cylinders were placed horizontally on a bench and butted against a masonry wall and the readings taken on a circular end, throwing out obviously high or low readings. Large aggregate at the surface will result in high readings and small air pockets will result in low readings. Seventy-five miscellaneous concrete cylinders being tested in the laboratory were also checked prior to being broken. The rebound numbers versus compressive strength for each end are plotted in Fig. S and T.

Several cylinders were removed from the compression machine as soon as the ultimate load had been reached and the indicator dial needle "fell off."

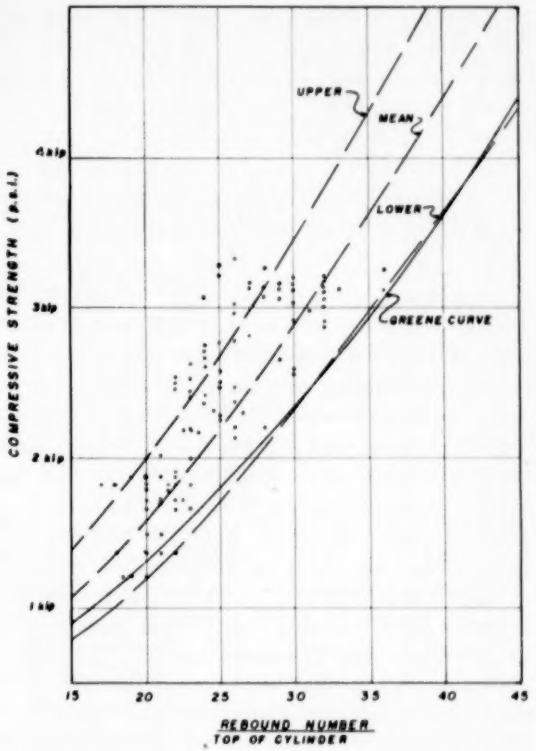


Fig. T—Rebound numbers versus compressive strength—top of cylinder

These cylinders were then checked for rebound number and the value was within the same limits as the readings before the cylinders were tested.

FLEXURAL STRENGTH TESTS

Thirty-six 6 x 6 x 24-in. concrete beams were tested in the laboratory before and after flexural tests. Rebound readings were taken on the top surface, each side, and bottom surface. The top surface was usually rather rough and average readings on this surface were from 5 to 10 percent less than those taken on the sides of the beam. The bottom of the beam resulted in values 10 to 20 percent higher than the readings taken on the sides of the beam. This is probably due to the bottom surface having fewer minute air pockets than occur along the sides, thus resulting in a more dense surface.

Plotting the results of these tests on Mr. Greene's plot of rebound number against modulus of rupture gave the same spread of results as reported by Mr. Greene. The average rebound number from the sides of the beam were used. It was noted that hammer readings taken within 4 in. from either end were consistently lower than those taken within the middle $\frac{2}{3}$ of the beam length.

The longest broken section of one series of 19 beams was allowed to dry for several hours and readings again taken. This portion of the beam was then placed in a water tank and allowed to become "surface wet" and readings again were taken. The "surface dry" condition produced results 1 to 2 percent higher on the average than the "surface wet" condition. Placing the same beams in water for 2 to 3 days and then allowing the surface of the beam to become dry resulted in an average 14 percent increase in rebound readings over the original surface dry readings.

AUTHOR'S CLOSURE

The author wishes to thank Messrs. Anderson, Bloem, Howard, Klieger, and Schlintz for their discussions.

The importance of conducting tests on a smooth surface, preferably one cast against a smooth form, can not be over emphasized. The suggestion by Mr. Bloem that the lower test results on his 10-in. beams could be due to the wood molds is probably correct. As mentioned in the original paper, results of tests conducted on a surface made against wood forms tested approximately 60 percent of the value obtained on a surface cast against smooth metal. A single large test specimen provided both surfaces.

Mr. Schlintz reports that tests conducted on the top or finished surface of a beam were 5 to 10 percent lower than tests conducted on the sides of the same beam. Mr. Klieger places the difference at 5 to 15 percent. Recent experience of the author in using the test hammer on similar beams agrees with these figures. This difference could be attributed to surface finish. Earlier tests by the author on beams made with 2½-in. aggregate showed higher tests on the top and bottom. However, this was due to the presence of large flat aggregate particles close to the surface.

The tests to determine the effect of specimen moisture on the rebound number conducted separately by Messrs. Klieger and Schlintz demonstrate the need for standardization of testing procedures with respect to specimen moisture.

It appears that the size, shape, and rigidity of the concrete under test affect the rebound number to a greater extent than previously realized. Mr. Anderson mentions that it is more difficult to obtain correlation between test hammer rebound and compression strengths using cylindrical specimens than with cubic specimens. He also emphasizes the need for rigid support of the test specimen. Mr. Bloem's tests show that specimen size and shape apparently affect the test result, although rigidity may also be a factor in these tests. The observation of Mr. Schlintz that rebound measurements made near the ends of a beam are lower than measurements made near the center seems to be related to the same problem of rigidity.

A test performed by the author since the original paper was written may cast some light on the subject of specimen rigidity. The test was designed to detect any difference in rebound numbers due to loading of the concrete

under test. Two 6 x 6 x 12-in. test specimens were made from ends of broken beams, each specimen representing a different concrete mixture. Each prism was loaded longitudinally with various unit stresses from 10 to 2000 psi in a compression testing machine. Six rebound measurements were made on each of the three sides which were made against a form and the 18 readings averaged. Fig. U shows the results. It appears that loading of a test specimen produces higher R values only because the specimen is more rigid.

The author has found that caution must be exercised when testing small concrete specimens with the hammer. For example, on one occasion the author tested an 8-in. wall with the hammer just prior to the cutting of 4-in. cores. Hammer tests made on the cores alone showed about one-half of the previous rebound. One core was placed in a 6 x 12-in. cylinder of fresh concrete so that the top of the core was flush with the top of the cylinder and the concrete was allowed to harden. The core surface when tested again showed nearly the same rebound as was obtained on the original wall. The concrete forming the matrix was purposely made much weaker than the concrete in the core.

Variations in rebound numbers with a given concrete compressive strength because of differences in aggregates were detected by Messrs. Bloem and Klieger. These results agree with the data presented by Schmidt and Herzig,* although the author has yet to find any significant differences due to aggregates alone, lightweight aggregates excepted. Measurements made on a polished concrete surface showed the mortar to give the same rebound as a smooth formed surface of the same concrete. The polished rock surfaces usually tested much higher, but varied from $R = 0$ for pumice to $R = 70$ for dense crystalline rocks such as granite, trap rock, quartzite, and amphibolite. The differences due to varying aggregate types probably can be made insignificant if the very high or low rebound numbers resulting from tests made directly over aggregate particles are disregarded.

The observation by Mr. Bloem that concrete made from 1-in. maximum aggregate produces lower rebound numbers than concrete of equal compressive strength made from 2-in. aggregate is probably related to this same

*See reference at end of original paper.

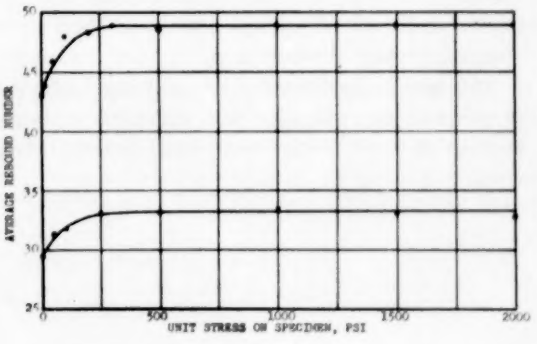


Fig. U—Rebound numbers as affected by loading the concrete specimen—two specimens

problem as mixes with larger aggregate usually require less sand and would have more large rock near the surface.

The objection raised by Mr. Klieger to the statement that compressive strength is proportional to rebound is justified. Certain qualifying words such as "within limits" or "approximately" should have been included. The hammer probably measures surface hardness more than any other property. Within limits set by age and surface smoothness the hardness is related to the concrete strength.

Mr. Klieger's comments on energy loss due to localized crushing and difference in viscosity or internal friction due to different aggregates and moisture conditions merit consideration. The calibration curves used with the test hammer can only compensate in an average way for these factors. If the test hammer must be used under unusual conditions where the average calibration curve is not sufficiently accurate, a new curve may be made to compensate for the unusual circumstances.

An examination of the various curves made by plotting average rebound against measured compressive strength does show a wide variation in strength for a given rebound number. It is well to bear in mind that some of the variation may be laid to the compression tests. When an average curve is used to convert rebound numbers to compressive strengths and the accuracy of the compression machine and the test hammer compared using a large number of specimens at a given strength, it is found that the compression machine is only slightly more accurate than the test hammer.

The test hammer is not a substitute for compression tests but a means of obtaining additional information concerning hardened concrete. The instrument provides a quick, inexpensive means of checking concrete quality. The comments by Mr. Howard show well how the instrument may be used.

The results of tests made using the hammer are going to be affected by: (1) smoothness of surface under test; (2) surface and internal moisture of specimen; (3) size, shape, and rigidity of specimen; (4) aggregate size and concentration near the surface under test; (5) aggregate type; and (6) age of specimen. The standardization of testing conditions and procedures will reduce or eliminate variations due to the first three causes. Special calibration curves or corrections applied to a standard curve would compensate for errors due to the last three causes.

The test hammer must be used carefully, recognizing the possible sources of error in test results. Mr. Klieger's statement that indiscriminate use of the test hammer might result in erroneous interpretation of results is certainly true.

Discussion of a paper by G. C. Ernst, R. R. Marlette, and G. V. Berg:

Ultimate Load Theory and Tests of Cylindrical Long Shell Roofs*

By K. HAJNAL-KÓNYI and H. TOTTENHAM, WILLIAM T. MARSHALL, ERIC C. MOLKE, and AUTHORS

By K. HAJNAL-KÓNYI and H. TOTTENHAM†

The writers would like to make a few comments on the tests described in the paper.

There seems to be a slight discrepancy in the dimensions in Fig. 4. The rise of the intrados of an arc with an inside radius of 30 in. and a clear span of 40 in. is 7.65 in. From the dimensions in Fig. 4 it would appear that this dimension is not less than $9.75 - 0.375 - 1.5 = 7.88$ in. The over-all depth/span ratio is $9.75:60 = 1:6.15$. In well over 200 practical applications of long barrel vaults known to the writers this ratio varies between 1:8 and 1:10. Ratios exceeding 1:7 are rare in "long" shells. Thus the models tested were not truly representative of average types of shells. An increase of the depth/span ratio increases the probability of failure in diagonal tension. However, of much greater influence is the method of loading. Generally, barrel vaults are used as roofs where the load is uniformly distributed over the whole span and maximum shear occurs only at the supports where the bending moment is zero. With third-point loading as applied in the tests under discussion the maximum shear is constant over the two external thirds of the span and at the third-point both the shear and the bending moment reach their maximum. Thus, in contrast to practical applications, the tendency to fail in diagonal tension instead of in bending was much enhanced by the conditions of the tests.

It seems to be doubtful whether distribution of the load in the transverse direction was really uniform. It may be assumed that the crosshead of the testing machine (Fig. 4) was rigid and that deformation of the ½-in. diameter loading rods was negligible. On the other hand, the vertical deformation of the shell must have varied along the arc so that even if the load distribution was uniform at the beginning of the test, in all probability it did not remain so as the load was increased. Without a pressure gage measuring the load in each rod there was no control of the load distribution. The peculiar shape

*ACI Journal, Nov. 1954, *Proc.* V. 51, p. 257. Disc. 51-12 is a part of copyrighted Journal of the American Concrete Institute, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

†London, England.

of the vertical deflection line within the elastic range is an indication of a variation of the load distribution in the transverse direction as the load increased.

According to Fig. 6, the load bearing capacity in bending of shell No. 3 (with the minimum amount of tensile reinforcement) was far from being reached at a total load of 12,000 lb, *i.e.*, 96 percent of the ultimate. The tensile and compressive strains (scaled) were 0.00048 and 0.00035, respectively. This indicates a tensile stress in the steel of the order of only 14,000 psi and a maximum compressive stress in the concrete of the order of only 1400 to 1600 psi. It is the more surprising that the bending moment produced by the external forces at failure exceeded many times the maximum bending moment that could have been developed by the tensile reinforcement. This will be seen from the following calculation.

The principal steel in each tension flange of shell No. 3 consisted of one ¼-in. round bar. With an assumed yield point of 40,000 psi, the total tensile force at yielding in the two flanges would have been 4000 lb. This could have been balanced by less than 1 sq in. of concrete at the crown. With an assumed cover of ½ in., the lever arm would have been $9.75 - 0.5 - 0.125 - 0.188 = 8.94$ in. and $M_{ult} = 8.94 \times 4000 = 35{,}760$ in.-lb.

The bending moment developed by the external forces at failure was $20 \times 6250 = 125{,}000$ in.-lb, *i.e., 3.5 times greater!* It is obvious that the longitudinal wires in the meshes must have made a substantial contribution to the bending moment of the internal forces. The arc length below the neutral axis in Fig. 6 measures approximately 7 in. Within this region there were $2 \times 2 \times 7 = 28$ wires of 15-gage with a sectional area of $28 \times 0.00407 = 0.114$ sq in., capable of developing a tensile force of $0.114 \times 55{,}000$ to $0.114 \times 64{,}000$, *i.e.*, 6250 to 7300 lb at "yielding," *i.e.*, much more than the tensile reinforcement in the flanges. If we assume the upper limit for the tensile force in the wires and a yield point of 50,000 psi in the two ¼-in. bars in the flanges, the tensile force at yielding is 12,300 lb. This is only three times the value assumed in the preceding calculation for M_{ult} with a substantially reduced lever arm. Thus it does not seem to be possible to eliminate the discrepancy between internal and external forces even if the tension in the meshes is taken into account. The discrepancy is, of course, much greater on the basis of the strains and the position of the neutral axis in Fig. 6.

Another surprising feature of the test is the magnitude of the load at first cracking. The extensibility of concrete is of the order of 0.0001 to 0.00015 so that one would have expected cracks to occur at a load of 4000 lb when the measured strain was 0.0002 (Fig. 6). However, the first cracks were observed at a load of 10,000 lb when the measured tensile strain was over 0.0004.

It would be desirable to have the authors' explanation on the points raised in the preceding.

The values in Table 1 confirm the influence of longitudinal reinforcement on maximum shear in accordance with many other tests on beams.[1,2]

Regarding the proposed method of design and with reference to Fig. 2 and to Example 1 in ASCE *Manual of Engineering Practice* No. 31, it should be pointed out that the omission of edge beams increases the transverse bending moments and may result in excessive deflections. The limitation of deflections is important in the design of large span shells. This is not possible if only ultimate load conditions are considered. Edge beams may be safely omitted at the internal valleys of shells which are continuous in the transverse direction but they must be provided at the unsupported external edges. The necessity of external edge beams is not apparent from short-time laboratory tests but would be drastically demonstrated if in practice large span shells were built in accordance with Fig. 2.

A further important factor which does not enter into short-time laboratory tests is the effect of creep. Experience has proved that the deflection of unsupported edges of large span shells after a few years may be three times its initial value.

The load of 10,000 lb at first cracking represents 80 percent of the ultimate load of shell No. 3. With a load factor of 2, this may lead to the expectation that shells remain crackless. However, it is a common feature of large span shells that they show a large number of hair cracks under their own weight. Such cracks are harmless as long as they do not exceed a certain limit, which is unlikely if deformed bars are used, but they do occur in practice in contrast to the authors' results on a small scale model.

To sum up:

1. The models tested are not characteristic of the majority of long shells as used in practice.
2. The method of loading adopted by the authors was entirely different from the actual loading of shells.
3. The transverse distribution of the load was in all probability not uniform and varied with increasing load.
4. It does not seem to be possible to reconcile the load bearing capacity in bending of shell No. 3 with the bending moment developed at failure by the external forces.
5. The discrepancy between internal and external forces is even greater on the basis of the strains shown in Fig. 6.
6. The measured strain at the load at first cracking is many times greater than the extensibility of the concrete.
7. Large span shells without external edge beams as suggested by Fig. 2 and Example 1 would show excessive deformations which may not be observed in short-time tests.

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2. Hajnal-Kónyi, K., "Recent Research on Deformed Reinforcing Bars," *The Reinforced Concrete Review* (London), V. 3, No. 7.

By WILLIAM T. MARSHALL*

I was extremely interested in the application by Messrs. Ernst, Marlette, and Berg of the plastic design theory developed by Prof. A. L. L. Baker and others. This method requires so much less time in design than the elastic theory that experimental evidence, such as that produced by the authors, to verify it is of great help to the engineering profession.

The writer would, however, like to comment on the following points.

Equation (8)—The form as given is correct when $\beta_1 > \beta_{NA}$ but scarcely practical since the critical section is the crown where $\beta_1 = 0$. At the crown the relevant equation is

$$M_{TV} = v'_{max}\, tr^2 \left[\beta_T - \frac{\beta_{NA}}{2} - \sin \beta_T + \frac{(1 - \cos \beta_{NA})}{\beta_{NA}} \right]$$

Equation (12)—This can be rewritten as

$$\left(\frac{r}{t}\right)_{max} = 54.3 + \frac{250{,}000}{f_c'}$$

It is obvious that in this form the formula cannot be true for it states that the weaker the concrete the greater the permissible r/t whereas the opposite must obviously be the case.

The writer has carried out a number of experiments on thin steel shells, 36-in. long, 0.022-in. thick, and 15-in. diameter, giving $r/t = 680$. Such shells were quite stable under ordinary loading and while it is true that ultimate failure was by buckling this did not occur until the super-imposed load was approximately 1 psi. If however the circular portion of the shell had ribs placed transversely, it was found that buckling failure did not occur.

The r/t ratio was the same in the case of both ribbed and ordinary shells and it is therefore clear that the unsupported length of the shell must be included in any criterion for longitudinal buckling.

The authors seem to have taken the transverse reinforcement in every case to be $0.015bt$, irrespective of the bending moment. This is unnecessarily wasteful and steel can be saved by simple ultimate design methods. With Whitney's theory for under-reinforced beams, the ultimate bending moment is approximately $0.9\ A_t f_y d$ or $A_t f_y\ [d - (n/2)]$, where d = depth to center of steel reinforcement. The writer therefore suggests that in the first instance n is assumed = $d/3$; the area of reinforcement is then calculated and the compressive stress in the concrete. It will, in general, be found that this is well below the specified limit of $0.85f_c'$. The transverse thrust has so far been neglected but it produces a uniform compression over the whole section. This stress when added to the previously calculated compressive stress due to bending will give a total stress which will again be found generally to be less than $0.85f_c'$.

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Applying this method to the example given by the authors:

M_T = 53,700 in.-lb per ft

Over-all t = 3.75 in., hence take d = 3.00 in.

Assume $n = d/3$ = 1.00 in.

Lever arm = $d - n/2$ (at ultimate load) = 2.50 in.

$$A = \frac{53{,}700}{40{,}000 \times 2.5} = 0.537 \text{ sq in. per ft, a saving of 25 percent}$$

$$c = \frac{40{,}000 \times 0.537}{12 \times 1.00} = 1790 \text{ psi}$$

Transverse thrust = 6520 lb per ft width

$$\text{Uniform } c = \frac{6520}{12 \times 3.0} = 182 \text{ psi}$$

Resultant compressive stress = 1790 + 182 = 1972 psi

Since this is so much less than 0.85 × 5000, it means that the assumed value for n is too high. Further approximations could be made until a value for the compressive stress nearer 4250 psi is obtained.

If, for example, n is assumed = 0.5 in., $d - n/2$ = 2.75 in.; A = 0.488 sq in. per ft; c = 3255 + 182 = 3437 psi.

Due to the approximate method of dealing with the problem of moment plus thrust it is inadvisable to proceed with any smaller value for n. The steel area of 0.488 sq in. per ft, however, represents a considerable saving on the 0.675 sq in. per ft obtained by the percentage method.

This transverse steel is difficult to place and every effort should be made to keep it to a minimum.

By ERIC C. MOLKE*

It is questionable whether an "ultimate load theory" for shells and high girders should use the same approach that is now customarily taken for shallow girders. In conventional beam theory we primarily consider longitudinal bending stresses for which we assume a straight-line distribution. When we refine this theory to investigate the condition at "ultimate load" we merely speculate about the distribution of the bending stresses above the neutral axis and are happy to find that the assumption of a rectangular stress block makes good sense. From this stress block we find a simple linear distribution of shearing stresses. This simplification is satisfactory for shallow beams and most structural elements are shallow.

When investigating shell structures, which in essence are high, sectionally curved, slender beams, we must speculate about the distribution of shearing stresses in the elastic as well as in the plastic ultimate stage.

The outstanding factor in the calculation of high beams is the angular deformation of a beam element, caused by the shearing forces, and this has been the basis of the elastic theory of high walls and shells. The depth/

*Prestressing Research & Development, Inc., San Antonio, Texas.

span ratio as well as arrangement of the load will influence greatly the distribution of these shearing stresses.

While the authors, in line with Lundgren's suggestions, confine themselves to an investigation of long shells, which more nearly lend themselves to the assumption of a rectangular stress block at failure, it seems doubtful that the resulting linear shear distribution as per Fig. 2 is satisfactory for the calculation of critical principal stresses. A. L. L. Baker recommends the use of a parabolic shear distribution near the supports, apparently as a correction to the authors' assumption.

Shorter shells need other corrections for the stress block assumption and it seems that what is gained by ultimate load theory in simplifying the design procedure is lost by the necessity of allowing for necessary corrections and limitations. Shells can be almost as quickly designed by elastic theory when using some simplifying assumptions. It is admitted, however, that the ultimate theory lends itself to a better understanding of the basic principles of shell design and performance.

Shells on account of their configuration are generally suited only for roof construction. A roof, much more than any other structure, must first be leakproof and consequently crackproof. While, naturally, there must be a factor of safety for ultimate collapse due to bending stresses, crackproofness and elastic stability are the principal factors in the design of a shell roof. Safety for cracking will best be proved in the elastic range and reinforcing should be distributed accordingly.

It seems that the authors' tests have proved in an interesting manner that the greatest role in the supporting action is played by the shearing stresses and the resulting principal stresses. Destruction of the shell originated at diagonal cracks near the support and, while it was important to have sufficient longitudinal edge reinforcement for bending stresses, the amount of web reinforcement was decisive.

The positive end anchorage of the edge bars might have been a favorable factor in the supporting action, allowing an arching action after probable bond failure of the bars, which would be quite different from the stress distribution assumed in Fig. 2 of the authors' paper. Similar beneficial arching action after bond failure might be obtained when using well anchored prestressing units along the edges.

AUTHORS' CLOSURE

The authors appreciate the questions raised by Messrs. Hajnal-Kónyi and Tottenham, particularly with regard to the exploratory tests. It was not realized that the brief description had been presented in an ambiguous manner, and it will no doubt be helpful to others to clarify the situation.

The dimensions in Fig. 4 are from measurements made after assembly of the forms, with the *nominal* radius of 30 in. shown, and hence the relatively slight difference between the theoretical value of 7.65 in. and the actual value of 7.88 in. for the rise. With regard to the depth/span ratio, as well

as the method of loading, the conventional testing technique of exaggerating the questionable factors was used to determine the limits of application. There would seem to be no point to testing an average or an extremely long shell. On the other hand, by selecting a depth/span ratio on the borderline between short and long shells, it is evident that if assumptions for long shells are met within acceptable limits, long shell theory is confirmed. The same reasoning holds true for the method of loading, with the added advantage that third-point loading subjects a portion of the shell to constant moment and a different portion to constant shear. Those acquainted with the basic concepts of structural testing undoubtedly recognized the above reasons for selecting the shell proportions and method of test. Contrary to the opinion of Messrs. Hajnal-Kónyi and Tottenham, distribution of load in the transverse direction was virtually uniform, due to the action of the compression springs operating independently between each loading rod and the crosshead of the testing machine, as stated and shown in Fig. 4. As may be recognized readily, vertical movement of the shell was insignificant in comparison with that required to reduce the spring load appreciably on any rod. The compression springs were included in the special testing head for just that purpose, which is also a rather routine testing method for maintaining a relatively constant load under conditions of deformation or deflection. The peculiar shape of the vertical deflection curve, instead of being due to variation of load distribution, is definitely caused by the exceptionally high shear which produced early diagonal tension cracking, as seems obvious from a comparison of the vertical and lateral deflection curves of Fig. 9.

The authors must apologize for the overemphasis on the term "principal" reinforcement in the tension flanges, which proved so misleading in the attempted comparison of external and internal moments. In the case of shell No. 3 the area of the mesh reinforcement contained in each tension flange was greater than the area of the ¼-in. bar because of the manner of construction of the shell. Inasmuch as the primary failure of all shells was due to diagonal tension, the actual steel content in the flanges was not discussed in sufficient detail to permit checking internal moment against external moment.

As stated on p. 264, it was expected that the steel in shell No. 3 might reach the yield point, and it is apparent that this was practically true at the ultimate load of 12,500 lb, although primary failure of the shell occurred in diagonal tension beginning at the load of 6000 lb as is evident from Fig. 9. For purposes of controlling the position of the mesh in the shell proper, the mesh was brought into the flange under the ¼-in. bar and up on the outside, and the excess returned by bending back into the flange. The mesh lap was wired together and pulled down taut by tie wires to assure that the mesh would maintain its position in the shell proper while placing the cement mortar. The mesh was 48 x 66-in. in over-all size, with the 48-in. length extending longitudinally from each end diaphragm, thereby producing a double thickness of mesh in the central 36 in. of shell span. Inasmuch as the

length of the arc from the ¼-in. bar in one flange to the ¼-in. bar in the other flange was approximately 50 in., it is apparent that each tension flange had about 2 × 10 = 20 longitudinal mesh bars for which the center of gravity was close to that of the ¼-in. bar. (This excludes from consideration that portion of the mesh acting in tension between the flange and the neutral axis.) The average yield point from eight tension tests on the mesh was 59,600 psi, and the yield point from one tension test on the ¼-in. bar stock was 48,300 psi. Therefore, the total flange tension at yielding would be close to 2 × 20 × 0.00407 × 59,600 + 2 × 0.05 × 48,300 = 9700 + 4830 = 14,530 lb. The internal moment at steel yield would be close to 14,530 × 8.94 = 129,900 in.-lb, not counting the mesh in the cracking up to 6 in. from the flange. Since the external moment at the ultimate load was 125,000 in.-lb, it is evident that the steel was not quite at the yield point. It is sincerely regretted if any readers were misled by the incompleteness in the description of the tension flange steel content, but the tests were not such that the tension flange steel was one of the critical variables.

With regard to tensile cracking and strains, early visual detection of flange cracking was inhibited by the effectiveness of the mesh in distributing the elongation and preventing early localization into visible cracks. This effect has been repeatedly demonstrated in tensile cracking tests. The increments of tensile strain in Fig. 6 give evidence of crack development as early as the 6000-lb load at which the concrete surface tensile strain was 0.00028, as discussed briefly on p. 265. Although 0.00028 is considerably higher than should be expected for plain or standard deformed bars, it seems entirely consistent when the strain distributing effect of the mesh in the flange is considered. In an effort to show the effect of the strain distribution, the maximum measured strain of 0.00047 was converted on p. 265 to an equivalent uncovered steel stress of 14,100 psi, which inadvertently implied that the steel was stressed to less than a third of its yield point. As a matter of record, a surface strain on the concrete (as measured in these tests) of 0.0001 would result in a maximum steel stress from 10,000 to 15,000 psi. With the mesh in the flange, incipient cracking would develop between 0.0002 and 0.0003 strain. At the maximum measured surface strain of 0.00047, the steel must have been close to the yield point stress at the cracks in the central portion of the shell flange, as the comparison of internal and external moments also indicated.

The authors appreciate the further confirmation of their statement on p. 265 that an increase in longitudinal reinforcement will increase slightly the ultimate capacity (and hence the maximum shear), as has been shown by previous beam tests by many other investigators.

The comments by Messrs. Hajnal-Kónyi and Tottenham with regard to the effect of omission of edge beams are quite correct. There are definite limits to the use of shells without edge beams. The senior author has presented elsewhere the full development for shells with edge beams, and also

a means for determining deflections,[1] which should prove helpful to those interested in the complete application of ultimate load theory to shells.

Professor Marshall's discerning discussion touches on several exceptionally pertinent matters concerning the application of ultimate load theory to the design of cylindrical shell roofs.

The form of the equation for M_{TV} as given by Professor Marshall contains the corrective term

$$-\frac{v'_{max}\, tr^2}{\beta_{NA}}\left[\frac{(\beta_{NA}-\beta_1)^2}{2}-1+\cos(\beta_{NA}-\beta_1)\right]$$

for the region above the neutral axis. The senior author has given this term in a more complete presentation of the theory as applied to a cylindrical shell with edge beams,[1] but also pointed out the negligible effect of the term due to the relatively high location of the neutral axis at ultimate load. For the example given in the paper, the correction amounts to less than 1 percent, and the neutral axis was restricted to $\beta_{NA} = 11$ deg 6 min. Actually, the use of Eq. (8) in the form presented amounts to assuming the value of v'_{max} to exist also in the small portion of the shell between the neutral axis and the crown, resulting in a slightly more conservative value for M_{TV}.

In developing inelastic buckling theory,[2] an empirical relationship between E_c and f_c' was used which would be applicable only within the range of concrete strengths from about 2000 to 6000 psi. The relationship is $E_c = 1{,}800{,}000 - 460\, f_c'$, which produces the form of Eq. (12). If the commonly assumed relationship $E_c = 1000\, f_c'$ had been used, Eq. (12) would have been $(r/t)_{max} = 118$. Although the concrete strength would control the magnitude of the load that could be carried by the shell, it has no significant effect upon $(r/t)_{max}$ since the limitation restricted the shell to the nonbuckling range. The authors are in substantial agreement with Professor Marshall in that the question of buckling is not a significant one for shells with ribs.

Although inelastic buckling under combined transverse thrust and moment is also unlikely to be critical, the authors wish to offer a word of warning for the commonly used procedure presented by Professor Marshall. The fact that a calculated compressive stress when added to a calculated bending stress yields a value below $0.85\, f_c'$ provides no assurance that inelastic buckling would not have occurred at an even lower stress than the combined one,[2] and certainly does not provide a balanced condition for ultimate design. It is dangerously incorrect, in the authors' opinion, to assume that superposition applies in the manner shown. Although the transverse steel ratio is definitely on the high side as shown by the fact that M_T produced by the load was smaller than the calculated value for ultimate capacity, it was pointed out under the section on "Combined transverse thrust and moment" that charts such as Fig. 36 could be constructed for other steel contents if further refinement were desired. It is conceded that the charts would probably be of greater practical use if not left in nondimensional form, but construction is simpler.

The authors are indebted to Mr. Molke for his comments, particularly in view of the rather condensed nature of the paper which did not include discussion of the points presented by him.

In evaluating the significance of various possible assumptions as to shear distribution in long shells, it was decided to select "borderline" test shell proportions and a loading method for which the effect of shear distribution would be magnified. Although the parabolic distribution near the supports is undoubtedly a truer representation, and would provide a more conservative approach, the results of the tests did not indicate the need for such refinenent. Obviously, the distribution assumed in the paper should not be projected to more extreme proportions or loadings than those used in the tests.

The authors are definitely in agreement with Mr. Molke's viewpoint that for shorter shells the elastic theory is practically as rapid as ultimate load theory. At the present state, the senior author prefers the elastic theory for short shells. However, as has been stated before, structures should be designed to function adequately in both elastic and plastic phases.

As Mr. Molke has pointed out, a considerable advantage may be gained in controlling stress distribution by prestressing the longitudinal edge steel. In fact, prestressing would appear to be a desirable method for assuring the desired distribution.

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Discussion of a paper by M. F. Macnaughton and John B. Herbich:

"Accidental" Air in Concrete*

By W. L. PUGH and E. W. SCRIPTURE, JR.

By W. L. PUGH†

Adequate field control of concrete construction has played an increasingly important part in construction projects during the past 20 years. Confidence that concrete will meet specifications has enabled the designer not only to effect economies but also to design for the most effective use of concrete materials.

Effective concrete control may be a fairly routine matter on most construction jobs, but in some instances unusual conditions arise which require careful attention of an extraordinary nature. The presence of "accidental" air in the fine aggregate around Lake St. John was such an unusual circumstance. This problem was solved satisfactorily, firstly, because it was recognized and identified at an early stage in the construction project, and, secondly, because the owners, the concrete control consultants, and the contractor were prepared to appreciate the unusual conditions and to make every effort to find a satisfactory solution and to put that solution into operation in the field.

Laboratory experimental work identified the trouble and this was confirmed in the field during the construction of temporary foundations for construction equipment. A widespread effort was made to obtain sand which would not contain this accidental air, but this comparatively easy solution to the problem proved to be impossible without adding considerably to the delivered cost of the fine aggregate. The laboratory work being carried out concurrently showed that the disadvantage of this unstable air could be overcome quite effectively at no great cost, so that this procedure was followed. The final conclusion of the authors of this paper follows the same reasoning as was employed with regard to the fine aggregate for the Peribonka jobs. The control of accidental and also entrained air in this concrete appears to have been entirely satisfactory as confirmed by field observations over a period of three years.

The authors are to be commended for their excellent paper and its clear description of this unusual condition. It should be beneficial to other members who may encounter similar conditions in the future.

*ACI Journal, Nov. 1954, *Proc.* V. 51, p. 273. Disc. 51-13 is a part of copyrighted Journal of the American Concrete Institute, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.
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By E. SCRIPTURE, JR.*

The authors have described a rather unusual phenomenon, occurring in concrete with certain sands, which has either not been recognized heretofore or has had only limited recognition. Their paper is especially helpful in that it gives a remedy for overcoming its detrimental effects. A most baffling aspect of the phenomenon is its unpredictability. I had the privilege of observing a demonstration in the laboratory at Peribonka wherein a plain mix with no addition whatsoever, after a fairly prolonged mixing period, developed an air content of about 14 percent. Since I desired to study this phenomenon further, the authors arranged for a shipment of this sand to our laboratories in a sealed steel drum, but unfortunately this sand, presumably from the same stockpile as that used in the laboratory demonstration, had completely lost its air-entraining qualities by the time it arrived in our laboratories. As the authors have noted, the air-entraining properties of the sand from a single source varied from day-to-day and with different conditions of storage and exposure. This made it all the more necessary to develop control procedures and materials which could correct the adverse conditions in all their variations.

In reporting on the use of an air-controlling agent the authors give some results secured with large quantities of this material, about 2500 times as much as was ever actually used in the construction work. While they mention this fact, it seems desirable to emphasize that quantities of this order of magnitude bear no relation to actual use.

In comparing the data in Tables 1 and 3 some misunderstanding might arise, since the paper does not point out that the results reported in these two tables were secured with entirely different additives. According to information furnished to me the results in Table 1 were secured with one specific air control agent and one specific air-entraining agent. The more satisfactory results reported in Table 3 were secured with a different air control agent and a different air-entraining agent combined with a cement-dispersing agent.

The test procedure for freezing and thawing is only briefly described in the paper, and no information is furnished regarding the rate of temperature change in these tests. It would have been of interest to include the rate of temperature change, since this condition greatly affects the results of freezing and thawing tests.

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Discussion of a paper by Hubert Woods:

Observations on the Resistance of Concrete to Freezing and Thawing*

By J. E. BACKSTROM and R. W. BURROWS and AUTHOR

By J. E. BACKSTROM and R. W. BURROWS†

Mr. Woods' recapitulation of the role of entrained air on the production of frost-resistant concrete covers the subject admirably. Many of the tests performed in the Bureau of Reclamation laboratories confirm his findings insofar as air entrainment is concerned. However, we have some rather extensive data which indicate the cement may play a significant part in the development of frost resistance.

It is agreed that the usual laboratory freezing and thawing test of air-entrained concrete containing a good quality aggregate almost invariably indicates adequate frost resistance regardless of the cement used, and thus confirms Mr. Woods' third conclusion. However, recent Bureau of Reclamation tests on concretes containing a highly feldspathic natural aggregate with reactive constituents show that a reduction in the fineness and the alkali content of the cement significantly increased the frost resistance of air-entrained concrete. These tests also indicate that the cement may influence the frost resistance of a mature, weathered concrete to a much greater extent than is indicated by the results of conventional freezing and thawing tests on the immature moist cured concrete.

In this series of tests, 25 cements were produced by grinding two good quality Type II clinkers containing 0.25 and 0.93 percent (sodium equivalent) alkalies to five different finenesses and then blending to produce five different alkali contents in each fineness, each of which was combined with the aggregate mentioned above. Two 3 x 6-in. cylindrical specimens were fabricated from each mix for each of the following cures:

Cure A: 28 days in the fog room at 73.4 F

Cure B: 14 days in the fog room
6 months at 50 percent relative humidity and 73.4 F
3 days in water at 73.4 F

Cure C: 14 days in the fog room at 73.4 F
12 months outdoors in the semi-arid climate of Denver, Colo.
3 days in water at 73.4 F

*ACI JOURNAL, Dec. 1954, *Proc.* V. 51, pp. 345. Disc. 51-17 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.
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After these curings, the specimens were subjected to the Bureau freezing and thawing test.[1] To determine the magnitude of expansions due to alkali-aggregate reaction, three companion bars were also cast, cured for 7 days in the fog room, and stored at 130 F in sealed containers above water.

Microscopical determinations of air void spacing factors were made using the method developed by T. C. Powers.[2] These spacing factors were found to be almost constant, ranging from 0.0045 to 0.0049 in., and provide assurance that test results were not significantly influenced by variations in the air void systems.

Although low void spacing factors were obtained with all concretes, their frost resistances were relatively poor, possibly due to the thermal and bonding characteristics of the feldspathic aggregate. Frost resistance of the moist cured concrete (Fig. A, cure A) decreased with increased alkali content and, to a lesser extent, with increased fineness. When the alkali content was lowered from 0.60 to 0.25 percent, it is difficult to attribute the marked improvement in frost resistance to a reduction of alkali-aggregate reaction, because companion bars (stored at 130 F) containing the lower alkali cements showed low expansions thus indicating a minor degree of reactivity (Fig. B, cure A). In addition, the same trend of lower frost resistance with higher alkalies has been observed with nonreactive cement-aggregate combinations in other Bureau tests (not reported) and by Bailey Tremper.[3] It appears that low-alkali cement may possess other desirable properties apart from its low reactivity.

Progressing to cure B (concretes which were fog cured and then dried indoors under controlled conditions), it is seen that these companion specimens exhibited the typical high frost resistance of air-entrained concrete which has been dried prior to freezing and thawing. Most of these specimens did not develop sufficient expansion to plot in Fig. A, cure B. The results from the curve might seem to support Mr. Woods' contention, since, regardless of the fineness and alkali content of the cements, all the concretes exhibited a degree of frost resistance which might be termed "adequate." However, the effects of alkali content and fineness are still apparent.

Progressing to cure C, it is significant that the dried specimens, which were stored *outdoors* for a year before testing, had become quite vulnerable to frost damage. These specimens, although quite dry at the end of the weathering period, failed to develop the high frost resistance of their companion specimens dried indoors (cure B). Apparently the moisture and temperature changes of outdoor exposure, coupled with the thermal and bonding properties of the feldspathic aggregate and the cracking and healing properties of the cements, produced different degrees of internal damage which greatly influenced the vulnerability of the concretes to freezing and thawing. The entrained air did *not* provide adequate protection. The factor of cement fineness, which appeared relatively insignificant with cures A and B, played a much more vital role under the more realistic conditions of cure C (stored outdoors). This effect has been previously noted in another investigation[4]

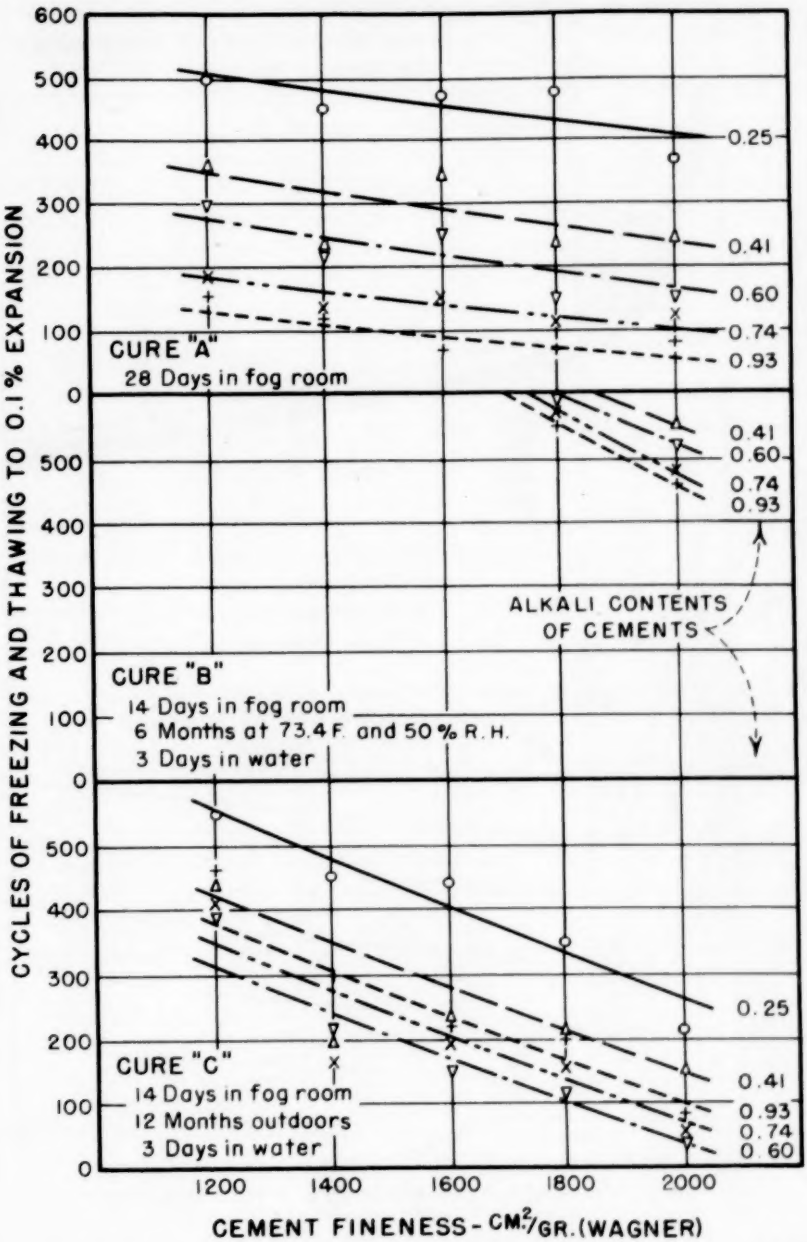

Fig. A—Fineness and alkali content of the cement significantly influence the frost resistances of these air-entrained concretes

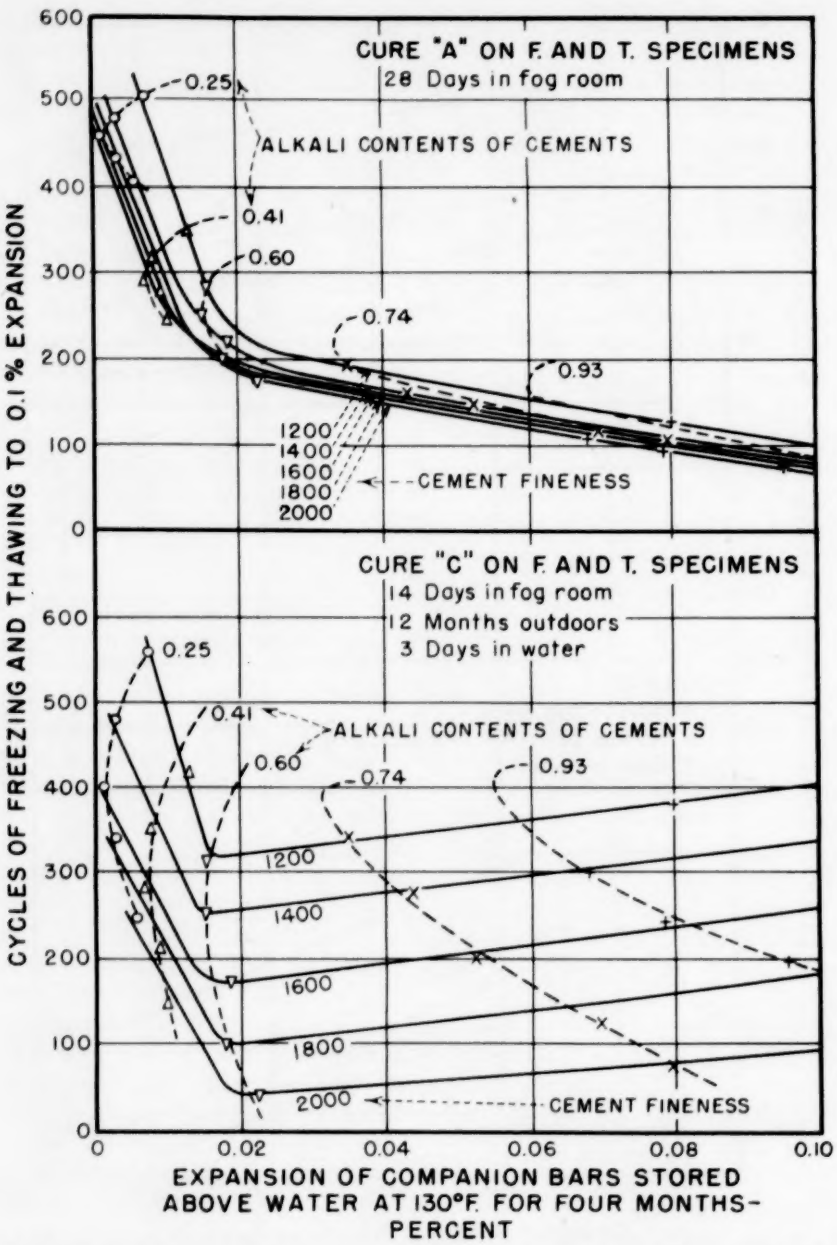

Fig. B—The large difference between the frost resistances of concretes having low reactive potentials indicate that factors other than expansion due to alkali-aggregate reaction influenced the frost resistances

and was attributed to the cracking and healing characteristics of the cement. Fig. B, cure C, shows that frost resistance of the concretes was not an implicit function of the amount of alkali-aggregate reaction, since concretes with the same degree of reactivity showed widely different frost resistances.

Much has to be learned about the role of the cement in making durable concrete. It has been our experience from an abundance of paradoxical test data, the above included, that laboratory freezing and thawing tests of continuously moist cured, immature, undried, and unweathered concrete tend to minimize the true role of the cement. In such tests, the cement has little opportunity to play its part, either as a bad actor or as a good one. In concrete exposed to natural weathering, the cement is called upon to resist the cracking influences of moisture and temperature changes, cement-aggregate incompatibilities, and alkali-aggregate reaction. In weathering, the mechanisms of internal cracking and autogenous healing occur together over a considerable period. Eventually, the internal structure of concrete may "adjust" to a reasonably stable and sound condition. A similar concept is

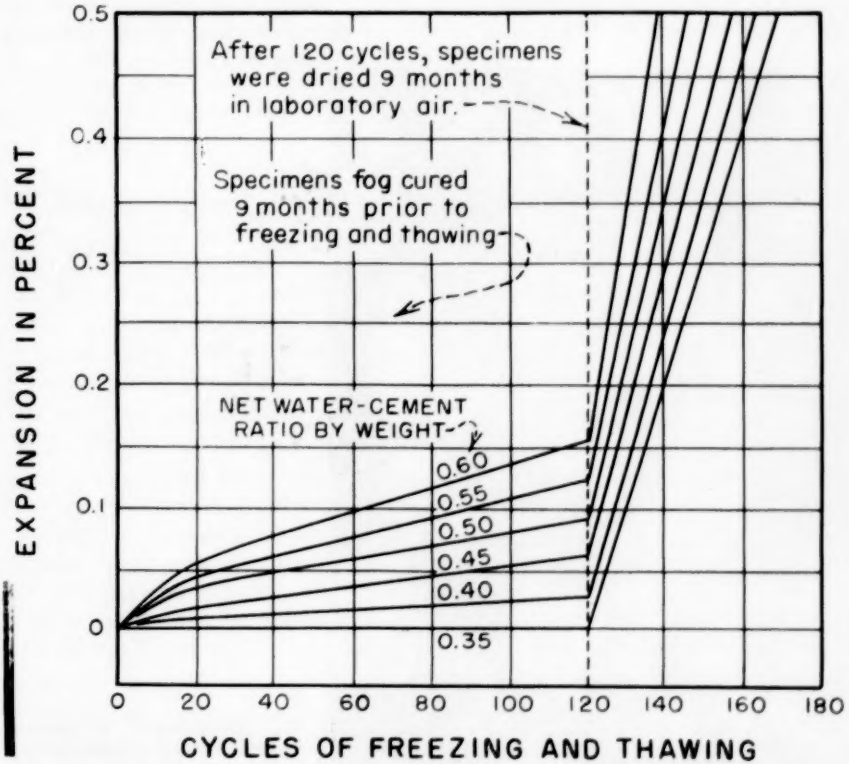

Fig. C—Low water-cement ratios provide non-air-entrained concrete with high frost resistance only when the concrete is kept continuously moist

apparently held by Edwin J. Callan who suggests[5] that durability may be a function of a given cement to develop a hardened product whose properties are compatible with those of the aggregate. It is known that internal cracking reduces the frost resistance of non-air-entrained concrete and that air entrainment will protect concrete with some degree of fracturing. Therefore, the question may be: Can air entrainment be depended upon to protect concrete from frost damage regardless of the degree of internal unsoundness which has accrued from the inability of a cement to become "conditioned" to its enrivonment?

Mr. Woods' conclusion No. 5 tentatively suggests a possible alternative to entrained air for producing frost-resistant concrete. This is the use of water-cement ratios of below 0.4 by weight. This suggestion is apparently based on the premise that such concrete, when well cured, contains no freezable water and can therefore be little affected by freezing. Our tests of non-air-entrained concrete show this to be essentially true *provided* the concrete never dries out. When dried for a relatively long period and briefly resoaked, even concrete with the 0.35 water-cement ratio was vulnerable to frost action (Fig. C). Other tests (not reported) involving different cements, and various curing and drying methods have confirmed the deleterious effect of drying

Fig. D—Example of internal shrinkage cracks formed by drying concrete made of good quality materials and well cured. Magnification 50 × (approximate)

non-air-entrained concrete. This effect is attributed to the formation of internal microscopic shrinkage cracks but is not observed when relatively *short* drying periods are used. Apparently the internal cracking is not sufficiently developed in a short, mild, drying period to be reflected by a decrease in the freezing and thawing resistance. Instead, a *short* drying period usually increases the frost resistance of the concrete, with a reversal occurring as the drying period is lengthened or made more severe. A photomicrograph of internal shrinkage cracking is shown in Fig. D. It is believed that future investigations of freezing and thawing deterioration should include some consideration of the causes and effects of internal cracking.

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AUTHOR'S CLOSURE

I am grateful for the thoughtful discussion by Messrs. Backstrom and Burrows. They do not agree completely with my third conclusion, which was: "No portland cement clinker composition or cement fineness within the present or past commercial range has any significant advantage over any other with respect to the resistance of well-cured concrete to freezing and thawing when water soaked." They present data intended to show that "a reduction in the fineness and the alkali content of the cement significantly increased the frost resistance of air-entrained concrete."

The interpretation of the particular tests cited by Messrs. Backstrom and Burrows is exceptionally difficult because of the complicating presence of a "highly feldspathic natural aggregate with reactive constituents." We are obviously not dealing here with a simple case of freezing and thawing. It is possibly an instance of damage from alkali-aggregate reaction, complicated by freezing and thawing, or perhaps an instance of damage from freezing and thawing complicated by alkali-aggregate reaction. The complications of a situation like this, including the question of the manner of test, discussed later, are so great as almost to defy rational analysis.

It may be appropriate here to reiterate some of the points I made in my paper, namely, that the role of entrained air is to protect the *paste*, and that entrained air cannot be expected to make bad aggregate good. The paste, with entrained air, could be resistant beyond reproach; but the concrete,

because of bad aggregate, could show poor resistance to frost action. I would therefore readily agree that with alkali-reactive aggregate causing internal damage related somehow to alkali content or fineness of cement, or to both, the results of subsequent freezing and thawing tests would appear also to be causally related to alkali content or fineness of cement, or to both. The discussors find it difficult to attribute the marked improvement in frost resistance of some of their specimens to reduction in alkali-aggregate reaction because of less alkali in cement (Fig. B, cure A). Nevertheless, their plot does show that the concretes which were most vulnerable to freezing and thawing *were* precisely those concretes which expanded most when tested for reaction at 130 F in a moist atmosphere for 4 months; and, conversely, the concretes which were least vulnerable to freezing and thawing *were* precisely those concretes which expanded least when similarly tested. Furthermore, the amount of expansion *was* related to the alkali content of cement. Some of the details of the test results are obviously not susceptible to easy explanation. However, the general features of the data are quite what one would expect—the concretes were variously damaged by expansion from reactive aggregate, and were therefore variously but consistently made more vulnerable to freezing and thawing.

The discussors state that "the same trend of lower frost resistance with higher alkalies has been observed with nonreactive cement-aggregate combinations in other Bureau tests (not reported) and by Bailey Tremper."[3] As the other Bureau results are not given, I cannot comment on them. The aggregate from Rock Island pit, used by Tremper, had such a poor service record when used with high-alkali cements as to provide extensive field evidence that it was in fact reactive whether so shown by the mortar bar test, or not. But even more significant is the fact that the reported effect of alkalies on frost resistance, as determined by laboratory tests, was observed by Tremper only on non-air-entrained concrete. Tremper says: "The addition of an air-entraining agent in amounts to bring the total air content of the fresh concrete to values between 3 and 6 percent was found to be effective in increasing the resistance to freezing and thawing to the point that high-alkali cements gave substantially as good durability as the low-alkali cement."

My paper did not contend that there were no differences to be found in the resistance to frost action of non-air-entrained concretes made with different cements. Such differences have been reported by many investigators, and have provided much of the "paradoxical" data which confront us. Without entrained air, concrete is vulnerable to freezing and thawing, and many factors affect the exact degree of vulnerability. Among these factors are the amount of freezable water in the concrete, amount and disposition of internal void space, and permeability. All these factors may well be in part dependent upon cement composition and fineness, the original water-cement ratio, the extent of moist curing, and the extent of drying and subsequent wetting. Also, internal damage, as from alkali-aggregate reaction, may be

important. Consequently, it is easy to see why, with all the variables which may be at work in such cases, various investigators have observed and reported various results, frequently quite opposite in nature.

The third conclusion of my paper ought to be read along with the first, namely, that "concrete of normal water-cement ratio, without entrained air, is vulnerable to freezing and thawing when water soaked." To produce frost-resistant concrete one should obviously use entrained air, and when this is done properly, cement composition and fineness (within the commercial range) lose the critical significance they may sometimes appear to have with respect to frost resistance of well-cured, non-air-entrained concrete. Naturally, if reactive aggregates are to be used, one should take whatever steps are needed to cope with that situation, and not expect entrained air to prevent damage. As my paper indicated, entrained air is not a cure-all.

The discussors conclude their discussion of my third conclusion with a question: "Therefore, the question may be: Can air entrainment be depended upon to protect concrete from frost damage regardless of the degree of internal unsoundness which has accrued from the inability of a cement to become 'conditioned' to its environment?" This question is difficult to answer directly because it assumes as proved, a hypothesis which says that cements have and need some special ability to "become conditioned to (their) environment." Presumably the hypothesis would also state that different cements have different amounts of this special ability. This is an interesting hypothesis, but it is only a hypothesis and not a proved fact. In a sense, then, the question is a "loaded" one in that it is dependent upon an unproved assumption. The question can be reworded to remove its hypothetical basis. Let us ask: Can air entrainment be depended upon to protect concrete from frost damage regardless of the degree of internal unsoundness which has accrued for any reason whatsoever? Now the answer clearly must be no. We should not expect anything so miraculous from air entrainment. There are several ways in which concrete can be damaged internally, resulting in lower frost resistance, in which case entrained air might or might not have some alleviating effect. But this subject is really outside the scope of my paper.

I do not pretend to be able to refute or to explain all the data presented by Messrs. Backstrom and Burrows, which they themselves refer to as "paradoxical." One of the principal difficulties facing anyone who tries to make sense of all the tests on freezing and thawing is the large amount of such "paradoxical" data which confronts him. Perhaps a chief culprit in having led to such a situation is the laboratory freezing and thawing test, or more accurately, tests, because there are several in use. Bearing only a superficial resemblance to natural freezing and thawing processes, they are sensitive to differences in curing procedures and in general to the exposure history of concrete specimens before the test proper starts. Some of them freeze and thaw so destructively that they more nearly answer the question: "How much artificial abuse will this concrete take?" than the question: "Will this

concrete prove resistant to natural freezing and thawing?" They are overload tests, not service tests.

Messrs. Backstrom and Burrows indicated some reservation regarding laboratory freezing and thawing tests of moist concrete. They said: "In such tests, the cement has little opportunity to play its part, either as a bad actor or as a good one." Nevertheless, it is precisely on such a test that they rely for evidence to show that my third conclusion was wrong. For example, after their cure C, involving 12 months' exposure outdoors near Denver, concrete specimens did poorly in their *laboratory* freezing and thawing test, and the order of failure seemed to correlate with cement fineness and alkali content. However, the generally poor performance of the specimens first stored outdoors might signify only that such outdoor curing could cause specimens to show up poorly if subsequently tested in *that* laboratory freezing and thawing test. It might *not* signify that a year's outdoor exposure of air-entrained concrete renders it poorly able to withstand further natural freezing and thawing. In fact, the field exposure evidence, not involving laboratory tests, is quite to the contrary.

Messrs. Backstrom and Burrows commented on my conclusion 5, which suggested that use of a water-cement ratio below 0.40 by weight might be a possible alternative to entrained air. The information in Fig. C certainly does not lend support to this suggestion. The use of such a low water-cement ratio would frequently be difficult and in most cases the use of entrained air would appear to be the best procedure.

Fig. D is of considerable interest in showing internal shrinkage cracking in concrete. I agree completely with the thought that the causes and effects of such cracking need further investigation.

Discussion of a paper by Paul William Abeles:

Static and Fatigue Tests on Partially Prestressed Concrete Constructions*

By K. HAJNAL-KÓNYI and AUTHOR

By K. HAJNAL-KÓNYI†

In a contribution to the discussion of Dr. Abeles' paper "Fully and Partly Prestressed Reinforced Concrete" 10 years ago,‡ the writer suggested a certain type of composite construction which he thought was particularly suitable for bridges in flat country. The present paper shows a change in terminology during this period, as may be seen by comparison of the titles of the two papers, but deals mainly with the type of construction suggested by the writer 10 years ago. It is gratifying to the writer that his suggestion has been so thoroughly investigated both for static and fatigue loading and that all specimens (except for the beams tested in 1949) have behaved so satisfactorily even under severe testing conditions. Failure by bond even at 93 to 95 percent of the maximum possible ultimate load is undesirable, but, as was pointed out by Dr. Abeles, these first test beams were not truly representative of site conditions so that in actual structures a 100 percent cooperation between the precast and in situ part of the structure may be safely expected. This has been confirmed by the later tests described in the paper.

The investigations by Dr. Abeles have proved that the writer was too conservative in suggesting additional reinforcement in the in situ part of the composite structure (Fig. E in previous discussion‡). The in situ concrete below the neutral axis is subject to tensile stresses as soon as any load is superimposed on the composite structure. Under working loads these stresses, calculated on the basis of a homogeneous cross section, may be high, well above 1000 psi. It was therefore natural to suggest the arrangement shown in Fig. E. However, in the light of the experimental evidence now available (see cross section of the 1951 test slabs, Fig. 3 in the present paper) there is no need for any additional reinforcement in the in situ concrete.

*ACI JOURNAL, Dec. 1954, *Proc.* V. 51, p. 361. Disc. 51-19 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

†Consulting Engineer, London, England.

‡ACI JOURNAL, Nov. 1945 Supplement, *Proc.* V. 41, p. 216-8.

AUTHOR'S CLOSURE

Dr. Hajnal-Kónyi's contribution gives a welcome opportunity of surveying the essential features of partially prestressed composite construction, and adding a few particulars about further fatigue tests carried out by British Railways in 1954. Dr. Hajnal-Kónyi's more conservative suggestion (see Fig. K of reference 3* in paper) formed the first step to the more economical solution (Fig. L*), as the author indicated in the closure to the 1945 paper.*

The various systems of prestressing were surveyed by the author in his 1945 paper. Some systems relate to methods of prestressing, whereas others demand, directly or indirectly, definite stress conditions. These conditions represent an essential feature of full and partial prestressing and are discussed here in more detail and compared with ordinary reinforced concrete.

1. Full prestressing was suggested by Freyssinet in 1927, the condition being that the section must be under permanent compression under working load. A minimum effective prestressing force was required. An objection because of too high steel stresses was thus avoided, since there was no possibility of cracking and consequently no danger of corrosion.

2. In 1931 Nichols suggested full prestressing for mild steel reinforcement by using a steel area greater than required in conventional reinforced concrete. He assumed a certain proportion of the prestressing force would remain effective, an initial prestress of 32,000 psi being suggested. The same minimum effective prestressing force would be required as with full prestressing according to Freyssinet; but the initial prestress would be reduced to zero due to shrinkage, creep, and elastic shortening of the concrete and the state of full prestressing would, therefore, be available only temporarily. Thus, this proposal represented more or less a possibility with little practical advantage.

3. Partial prestressing according to the late F. v. Emperger consisted of additional tensioned wires (preferably arranged in bonded twisted pairs) to supplement the main conventional reinforcement as used in ordinary reinforced concrete. It was his intention to improve the behavior by means of the additional tensioned wires and to allow higher permissible stresses in view of the reduced danger of cracking. Such construction appears to be acceptable since in a cracked section the stresses will not be increased as much as in a prestressed member containing only high tensile wire. Referring to the "additional reinforcement," Emperger limited the proportion of the tensile force to be taken up by this additional steel, which might be considered to be appreciably less than half the entire tensile force, say one-third.

4. With partial prestressing according to the author's suggestion, the entire tensile reinforcement is of high tensile steel and either a part is tensioned and a part nontensioned or the entire reinforcement is tensioned so much as necessary to obtain the required prestressing force.

*Discussion of "Fully and Partly Prestressed Reinforced Concrete," ACI JOURNAL, Nov. 1945 Supplement, *Proc.* V. 41, pp. 216-1–216-25.

The working load stress diagrams of Fig. A show the essential difference between "full" and "partial" prestressing. Strictly speaking, only stress distributions (a) and (b) comply with the condition that only compressive stresses occur under working load. However, the practical application has been further developed to allow small tensile stresses below the level of the tensioned steel. The maximum value of these tensile stresses may be taken as 200 psi, when full freedom from any kind of cracking can still be insured. With partial prestressing this cannot be ensured if the concrete tensile stress calculated for a homogeneous section is in excess of the direct tensile strength of the concrete; but visible cracking can be prevented if this tensile stress does not reach the modulus of rupture. The difference between visible and microscopic cracking will be discussed later. Thus, for partial prestressing according to the author's proposal, a minimum permissible concrete tensile stress of 250 to 300 psi may be taken into account. Obviously the effective prestressing force required to insure a stress distribution according to Fig. A is greatest with (a) and smallest with (d).

Computation of the required effective prestressing force for a symmetrical cross section and a triangular stress diagram (Fig. B) is as follows. To simplify the matter, the counterbending of the dead weight is ignored and it is assumed that in the beam with pre-tensioned wires no tensile stress due to prestressing should occur at the top fiber to allow easy handling (*e.g.*, lifting the beam from the center). In such a case, the prestressing force applied at the kern K should be of the magnitude $(A_c f_{1e})/2$, where A_c is the cross-sectional area and f_{1e} is the required effective pre-compression at fiber 1 to counteract the stress due to loading. It is preferable to apply the major part of the prestressing force P_e (section area A_{st}) near the bottom fiber and to balance it by a tensioning force P_e' near the top $[P_e + P_e' = (A_c f_{1e})/2]$. The two forces P_e and P_e' can be obtained independently of the eccentricity of the kern e_K, as the author showed elsewhere,* by $P_e = (I_c k_2' f_{1e})/(a_s D)$

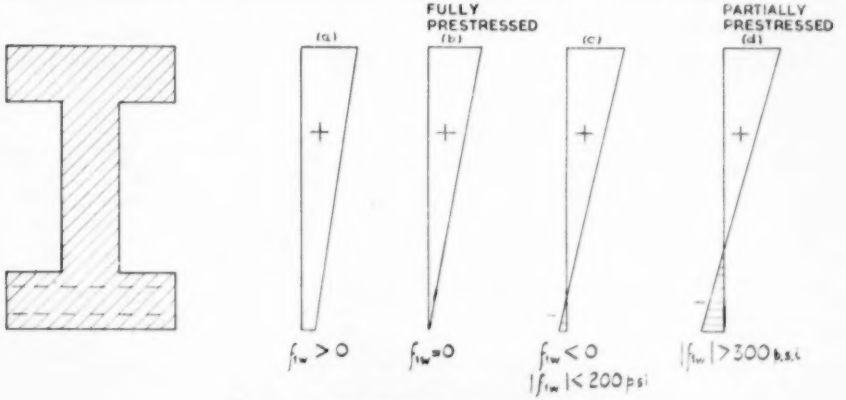

Fig. A—Resultant stresses at working load

*Abeles, P. W., *Principles and Practice of Prestressed Concrete*, Frederick Ungar Publishing Corp., New York, 1949, p. 30.

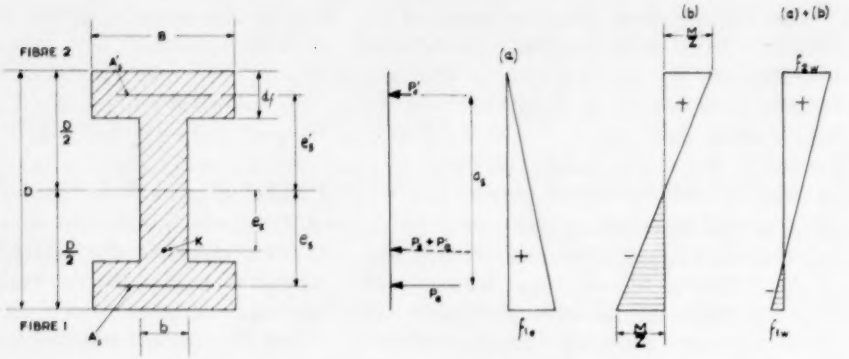

Fig. B—Effective prestressing force for symmetrical beam

and $P_e' = \dfrac{-k_2}{k_2'} P_e$, where $k_2' = 1 + \dfrac{e_s' D}{2r^2}$ and $k_2 = 1 - \dfrac{e_s D}{2r^2}$; $\left(r^2 = \dfrac{I_c}{A_c}\right)$.

Based on these considerations, various examples in prestressed and ordinary reinforced concrete are shown in Fig. C. For the sake of simplification rectangular sections 12 in. wide but of varying depth were designed to carry a working load bending moment of 1000 in.-kips. It must be pointed out that a rectangle is the least suitable section for prestressed concrete except for a shallow slab, but it was chosen for this comparison since in reinforced concrete a much deeper compression flange would have been required and this would have complicated the comparison. Two types of ordinary reinforced concrete were investigated: high-strength steel with high-strength concrete, example (c)—(f_s = 30,000 psi, f_c = 1688 psi, and n = 8), and ordinary reinforced concrete, example (e)—(f_s = 20,000 psi, f_c = 1125 psi, and n = 12). The same compressive stress f_c = 1688 psi was considered for f_{1c} in prestressed beams (a) and (b) corresponding to a stress at transfer of approximately 2200 psi.

Considering now the fully prestressed beam (a), the depth D is computed

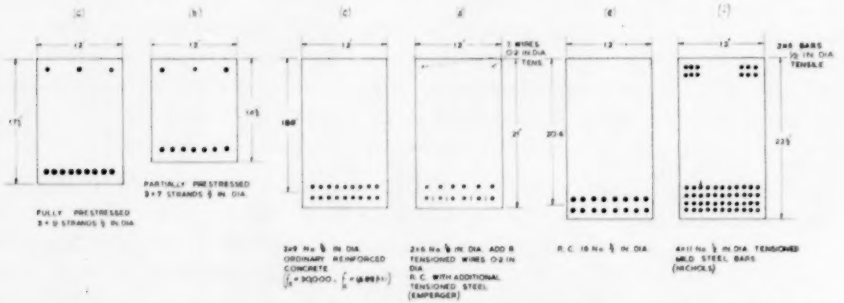
FULLY PRESTRESSED
PARTIALLY PRESTRESSED
2x9 No. 9 IN DIA. ORDINARY REINFORCED CONCRETE
2x6 No. 9 IN DIA. ADD 8 TENSIONED WIRES 0·2 IN DIA. R.C. WITH ADDITIONAL TENSIONED STEEL (EMPERGER)
R.C. 16 No. 9 IN DIA.
4x11 No. 9 IN DIA. TENSIONED MILD STEEL BARS (NICHOLS)

Fig. C—Comparison of rectangular beams capable of carrying the same working load bending moment of 1000 in.-kips

from $M = (BD^2/6)f_c$, resulting in $D = \sqrt{6M/Bf_c} = 17.3$ in., say $17\frac{1}{2}$ in., and the required prestressing forces are $P_e + P_e' = [(12 \times 17.5)/2]1688 = 177{,}000$ lb. Twelve stranded cables $\frac{1}{2}$ in. in diameter (sectional area 0.15 sq in.) have been considered, three at the top and nine at the bottom.

For partially prestressed beam (b) a concrete tensile stress of 700 psi under working load has been considered permissible. Consequently, the range of stress is increased by 700 psi to 2388 psi (*i.e.*, over 40 percent), and the required depth is $D = (\sqrt{1688/2388})17.3 = 14.5$ in. Not only depth but also required steel area is reduced in proportion with concrete area and only ten stranded cables are required (three at the top and seven at the bottom). In many cases of partial prestressing, required area of tensioned steel computed for permissible steel stress is less than the total area required for ultimate load conditions. However, with this rectangular beam this does not occur since the area A_c is relatively great and even for partially prestressed beam (b) the tensioned area steel A_{st} is greater than that required for ultimate load.

Example (d), partial prestressing according to Emperger, has been co-ordinated to the reinforced concrete beam (c) of high-strength materials. Two-thirds of the tensile force (*i.e.*, 12 bars $\frac{3}{8}$ in. in diameter) have been considered as the "main" reinforcement and the other third has been replaced by eight tensioned wires 0.2 in. in diameter. A permissible working load stress of 80,000 psi has been assumed, resulting in an average stress of $2/3 \times 30{,}000 + \frac{1}{3} \times 80{,}000 = 46{,}700$ psi.

Nichols' proposal of full prestressing (f) relates to low concrete stresses and has consequently been co-ordinated with beam (e) designed for a concrete stress of 1125 psi. Nichols recommended a percentage of reinforcement $p = 100f_c/32{,}000 = 112{,}500/32{,}000 = 3.5$ percent or 7.8 sq in., for which 44 pre-tensioned bars $\frac{1}{2}$ in. in diameter have been considered. Most likely

TABLE A—COMPARISON OF EXAMPLES (a) TO (f) IN FIG. C*

Example		a	b	c	d	e	f
D	in.	$17\frac{1}{2}$	$14\frac{1}{2}$	21	21	23	23
A_c	sq in.	210	174	252	252	276	276
	percent	121	100	146	146	157	157
A_s	sq in.	1.35	1.05	1.98	1.57	2.94	7.8
f_s†	psi	64,000	83,000	30,000	46,700	20,000	7,500

*Designed for a working load bending moment of 1000 in.-kips.
†Computed for a cracked section ignoring the prestress.
NOTE: Beams (c) and (e) are in ordinary reinforced concrete and cracking cannot be avoided. With the fully prestressed beam (a), containing high-strength wires, freedom from any kind of cracking can be insured. With the partially prestressed beam (b), also containing high-strength wires, freedom from visible cracking can be insured, though harmless microscopic cracks may develop. In the partially prestressed beam (d), hair cracks may occur which will close completely on removal of the load. With the prestressed beam (f), containing mild steel bars pre-tensioned to 32,000 psi, it is doubtful whether freedom from cracks can be insured though it is fully prestressed and the concrete stress is relatively low.

the entire initial tensioning stress will disappear in view of the losses, which may reach 35,000 psi, but visible cracks need not necessarily occur if the concrete is good, since the working load stress in the steel for a cracked section is only 7500 psi, all shrinkage having been taken up by the steel. Nevertheless, cracks may develop since the bending tensile stress under working load is approximately 800 psi. Thus, this is not a practicable solution.

Table A compares results and working load steel stresses are shown for the examples of prestressed beams, which stresses would occur if the tensioning were ignored and any cooperation of the concrete tensile zone neglected. Tensioned steel at the top has been assumed also in the prestressed members (d) and (f), as indicated in Fig. C. A top reinforcement would obviously be required in beams (c) and (e) for placing the stirrups.

Fig. D shows composite partially prestressed bridge slabs as used successfully by British Railways (Eastern Region) for the past 6 years, as well as some new developments. The precast prestressed beams with pre-tensioned wires, which are designed as light as possible for economy and ease of handling, support the added concrete; the composite section as a whole carries the additional load (road surfacing and live load). For spans exceeding 45-50 ft it is preferable, in the interests of economy, to reduce the dead weight by adopting some kind of hollow construction. A box shaped precast section would appear most suitable.

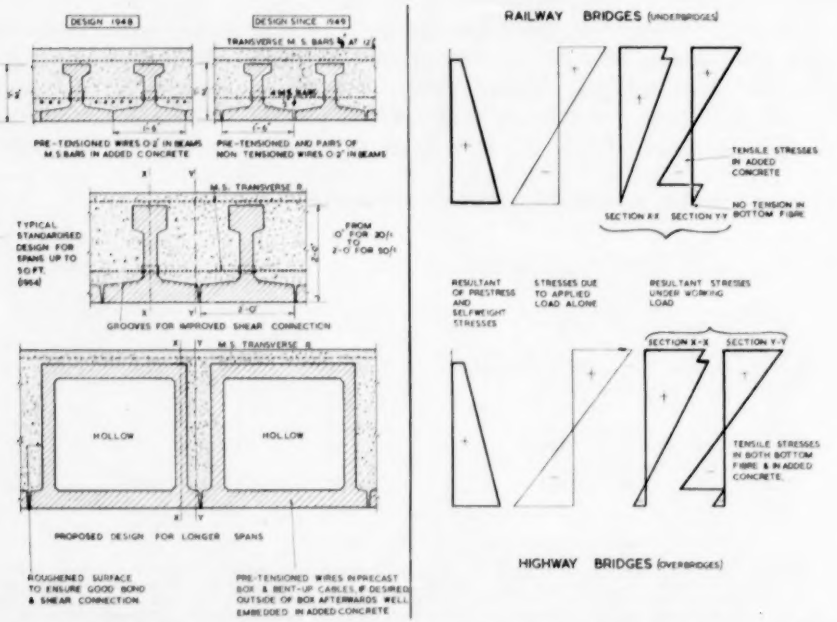
DESIGN 1948
DESIGN SINCE 1949
TRANSVERSE M.S. BARS
PRE-TENSIONED WIRES 0.2" IN BEAMS. M.S BARS IN ADDED CONCRETE
PRE-TENSIONED AND PAIRS OF NON TENSIONED WIRES 0.2" IN BEAMS
M.S. TRANSVERSE B
TYPICAL STANDARDISED DESIGN FOR SPANS UP TO 50 FT. (1954)
FROM 0' FOR 30' TO 2'-0" FOR 50'
GROOVES FOR IMPROVED SHEAR CONNECTION
HOLLOW
HOLLOW
PROPOSED DESIGN FOR LONGER SPANS
ROUGHENED SURFACE TO ENSURE GOOD BOND & SHEAR CONNECTION
PRE-TENSIONED WIRES IN PRECAST BOX & BENT-UP CABLES, IF DESIRED, OUTSIDE OF BOX AFTERWARDS WELL EMBEDDED IN ADDED CONCRETE
RAILWAY BRIDGES (UNDERBRIDGES)
TENSILE STRESSES IN ADDED CONCRETE
SECTION X-X
SECTION Y-Y
NO TENSION IN BOTTOM FIBRE
RESULTANT OF PRESTRESS AND SELFWEIGHT STRESSES
STRESSES DUE TO APPLIED LOAD ALONE
RESULTANT STRESSES UNDER WORKING LOAD
SECTION X-X
SECTION Y-Y
TENSILE STRESSES IN BOTH BOTTOM FIBRE & IN ADDED CONCRETE.
HIGHWAY BRIDGES (OVERBRIDGES)

Fig. D—*Left*: Typical composite bridge deck construction as employed by Eastern Region of British Railways. Right: Typical stress diagram for composite bridge deck

The precast beams contain well-bonded pre-tensioned wires. In highway bridges certain tensile stresses are allowed in the bottom fibers under working load, whereas in railway bridges, where there is greater impact, no tensile stresses are permitted in the bottom fibers of the precast members. But in both cases tensile stresses occur in the added concrete (see section $Y-Y$), which cooperates over its entire cross section and is restrained from visible cracking in view of the limited elongation of the adjacent prestressed part which is still in compression.

The same value for Young's modulus of elasticity can be assumed for both concretes if the added concrete is well vibrated. Stresses due to differences in shrinkage and creep of the two concretes are small and may be neglected. Provided that there is good bond between the added concrete and the precast prestressed beams, the composite deck will act as a monolithic slab. Stresses can then be superimposed as if entire section were prestressed.

Obviously, strict supervision is essential for obtaining agreement between the assumed effective prestressing force and the actual force in the structure. If this is insured, full use can be made of the properties, as a recent close inspection during a loading test of two bridges has proved, after these bridges had been in use for several years. If such an agreement is not insured a high factor of safety or reduced permissible stresses are of no use.

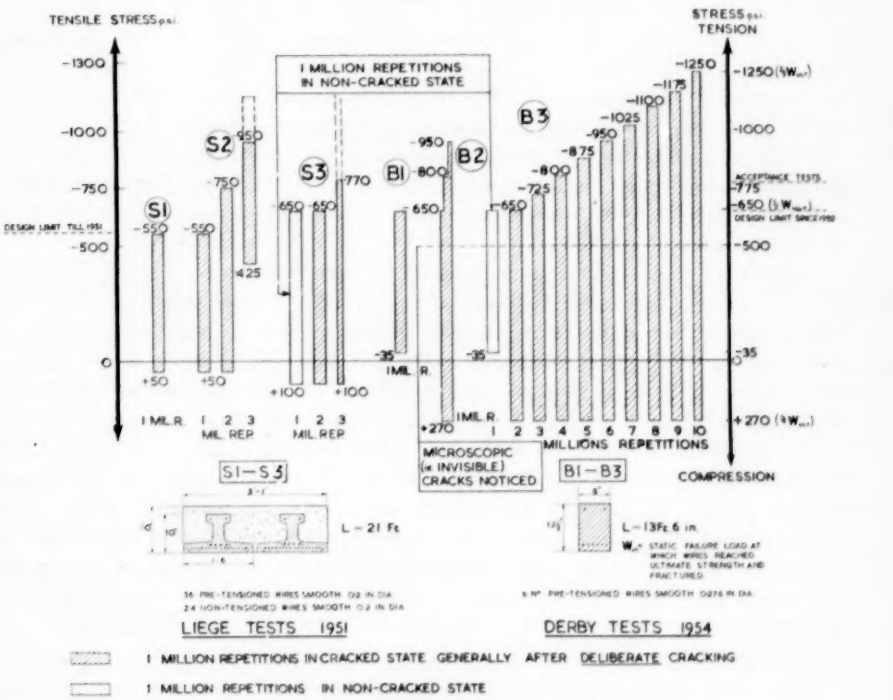

Fig. E—Range of stresses under fatigue loading

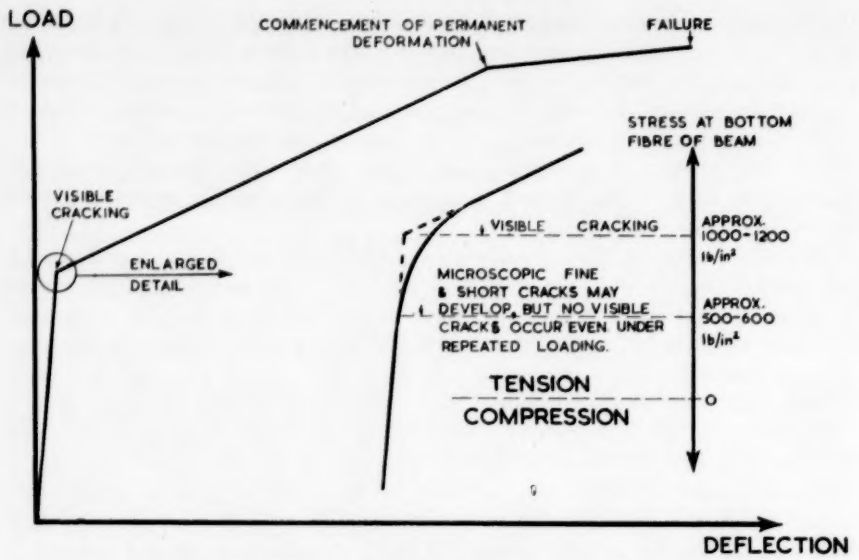

Fig. F—Microscopic (invisible) and visible cracking in prestressed concrete beams

Fig. E gives a survey of the fatigue tests carried out by British Railways. In addition to the Liége tests described in the paper, three tests were carried out in 1954 by the Railway Research Department, Derby, on rectangular beams with pre-tensioned smooth wires 0.276 in. in diameter.

At these Derby tests it was noticed from strain measurements that apparently microscopic cracks occurred before visible cracking at a load corresponding to the direct concrete tensile strength, *i.e.*, approximately half of the modulus of rupture.

From Fig. F it is seen that apparently such microscopic cracks develop when the load deflection line becomes curved. These small, narrow, invisible cracks are apparently entirely harmless and do not affect full cooperation of the entire composite section. The Derby tests have shown, as seen from Fig. E, that these microscopic cracks became visible only when the range of fatigue loading was increased (after completion of the first million load repetitions on B2), or when the static loading was greatly increased to a load corresponding to a tensile stress of 1270 psi, the cube strength being 9500 psi (beam B3). It is remarkable that it was impossible to notice any visible cracks even after these microscopic cracks had opened and closed a million times, but cracks were noticed immediately on increase of the range of fatigue loading. From strain measurements it can be concluded that apparently these microscopic cracks do not extend through the member; one can assume that a number of independent short cracks develop.

Thus, these latest test results and the practical experience during the last 6 years have proved the suitability of the author's proposal presented in 1945.

Discussion of a paper by Felix Candela:

Structural Applications of Hyperbolic Paraboloidical Shells*

By RAYMOND J. STITH and AUTHOR

By RAYMOND J. STITH†

The equation of the hyperbolic paraboloid usually given in analytic geometry texts is

$$\frac{x_1^2}{a^2} - \frac{y_1^2}{b^2} = 2cz \quad \ldots\ldots (1)$$

where x_1, y_1, and z are measured along the X_1, Y_1, and Z axes, and a, b, and c are constants. But for convenience the X_1 axis may be rotated through a clockwise angle of $\omega/2$ to the X axis, and the Y_1 axis may be rotated through a clockwise angle of $90 - \omega/2$ to the Y axis (Fig. A), the rotations being in the X-Y plane. The angle ω may be defined such that the resulting X and Y axes will be the loci of the point $z = 0$ on the surface. Because of this definition,

$$\frac{x_1^2}{y_1^2} = \frac{b^2}{a^2} = \tan^2 \frac{\omega}{2} \quad \ldots\ldots (2)$$

Transforming the coordinates of any point P (Fig. A),

$$\left.\begin{aligned} x_1 &= y \cos\frac{\omega}{2} + x \cos\frac{\omega}{2} \text{ or } x_1^2 = (x+y)^2 \cos^2\frac{\omega}{2} \\ y_1 &= y \sin\frac{\omega}{2} - x \sin\frac{\omega}{2} \text{ or } y_1^2 = (x-y)^2 \sin^2\frac{\omega}{2} \end{aligned}\right\} \quad \ldots\ldots (3)$$

Now substituting Eq. (3) into Eq. (1), and multiplying through by b^2, we get

$$\frac{b^2}{a^2}(x+y)^2 \cos^2\frac{\omega}{2} - (x-y)^2 \sin^2\frac{\omega}{2} = 2cb^2z \quad \ldots\ldots (4)$$

and substituting Eq. (2) into Eq. (4),

$$(x+y)^2 \sin^2\frac{\omega}{2} - (x-y)^2 \sin^2\frac{\omega}{2} = 2cb^2z$$

which can be reduced to

$$4xy \sin^2\frac{\omega}{2} = 2cb^2z$$

Collecting the constants (except for the function of ω),

$$z = \left(\frac{2}{cb^2}\right) xy \sin^2\frac{\omega}{2} = k_1 xy \sin^2\frac{\omega}{2} \quad \ldots\ldots (5)$$

*ACI JOURNAL, Jan. 1955, *Proc.* V. 51, p. 397. Disc. 51-20 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

†Instructor in Civil Engineering, University of Dayton, Dayton, Ohio.

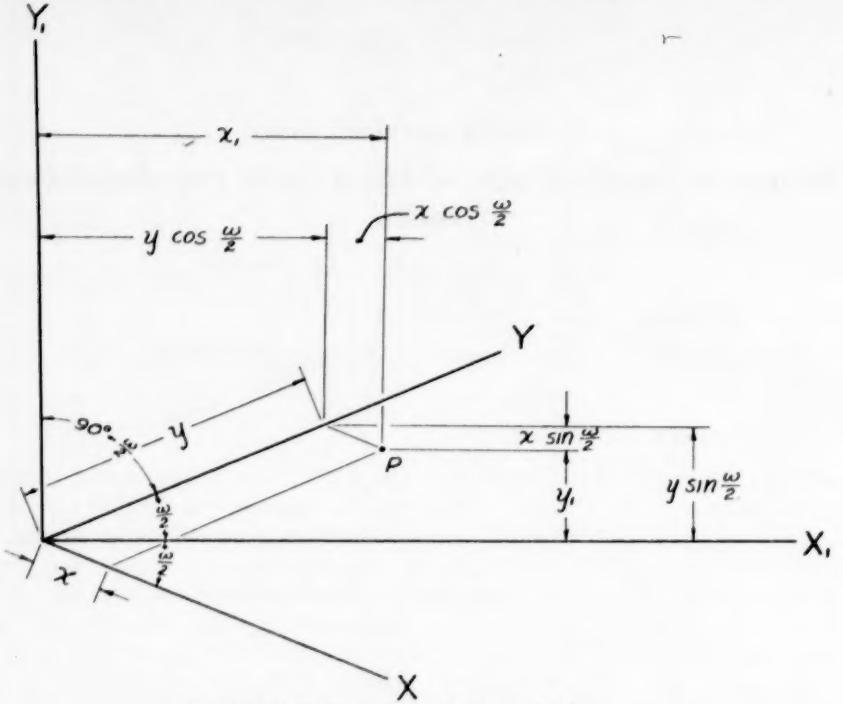

Fig. A—Rotation of the axes in the X-Y plane

which is similar to Mr. Candela's Eq. (1) except for the function of ω. However, although it is necessary to specify k_1 and ω to completely define the surface, ω is constant for any given surface, as is k_1, and it seems convenient to combine these two constants, thereby reducing the equation to

$$z = k_2 xy \quad \ldots\ldots (6)$$

It should also be noted that the constant is dimensional, the dimension being the reciprocal of length. Therefore it might be more convenient to change the equation to the form

$$z = \frac{1}{n} xy$$

which is the exact form of the equation given by K. G. Tester in Mr. Candela's bibliography item No. 10. Then the constant will be in units of feet, meters, or like units.

Because k is dimensional, it is meaningless to say that when $k<0.04$, uniform load on the horizontal projection can be substituted for dead load. But even if the dimensional unit of the constant is given, the constant does not of itself define the slope of the surface. For example, considering a warped parallelogram of square plan (Fig. B), the equation of the surface is $z = kxy$.

Use $k = 0.04$ ft^{-1}. Then if $a = 10$ ft, $c = 4$ ft, and $c/a = 0.4$; but if $a = 100$ ft, $c = 400$ ft, and $c/a = 4.0$.

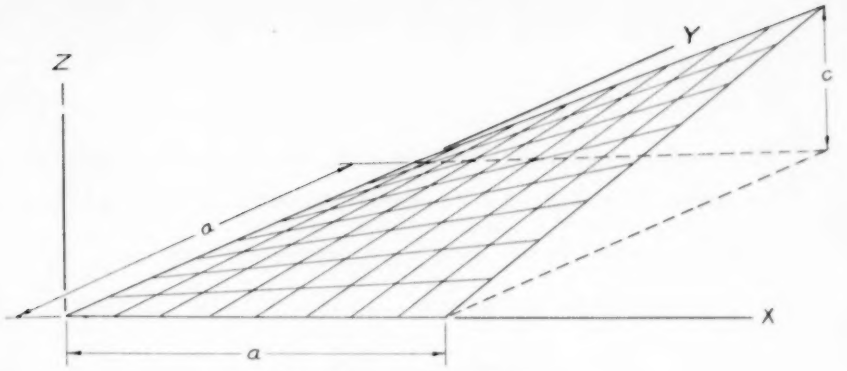

Fig. B—The warped parallelogram

Therefore it seems that Mr. Candela's criterion is not sufficiently descriptive of a flat surface.

AUTHOR'S CLOSURE

The author wishes to express his appreciation of Mr. Stith's analysis.

Mr. Stith's Eq. (5) is in fact the same as Eq. (1) of the original paper. Employing his notation we have:

$$\sin \omega = \frac{2ab}{a^2 + b^2}$$

$$\sin^2 \frac{\omega}{2} = \frac{b^2}{a^2 + b^2}$$

$$\frac{\sin^2 \frac{\omega}{2}}{b^2} = \frac{1}{a^2 + b^2} = \frac{\sin \omega}{2ab}$$

Substituting in Eq. (5) of the analysis by Mr. Stith:

$$z = \frac{2}{c} xy \frac{\sin^2 \frac{\omega}{2}}{b^2} = \frac{1}{abc} xy \sin \omega = kxy \sin \omega$$

The author agrees that the consideration of the value of k is meaningless as a criterion for the possible simplification of loads. It is better here for the designer to use his common sense.

The value of k is, nevertheless, sufficiently descriptive of the surface curvature or flatness, as it defines the *unitary warping* of the surface, that is, the vertical distance from a given point of the surface to the plane tangent at the surface in a point whose x, y coordinates differ unit-wise from the coordinates of the first point. This is the definition of unitary warping or unitary slope, and it does not refer, as Mr. Stith suggests, to the slope of the generators which is indeed variable, increasing with the distance of the generator to the origin of coordinates.

Discussion of a paper by L. F. Gleysteen and G. L. Kalousek:

Simplified Method for the Determination of Apparent Surface Area of Concrete Products*

By T. C. POWERS, STEPHEN BRUNAUER and L. E. COPELAND, and AUTHORS

By T. C. POWERS, STEPHEN BRUNAUER, and L. E. COPELAND†

Measurement of specific surface by vapor adsorption has proved fruitful in research on hydrated cement in this laboratory. We are pleased that Dr. Gleysteen and Dr. Kalousek, among others, are using this technique. But there is reason to question some of the results obtained by the rapid procedure proposed in this paper.

Basis for questioning may be seen in Table 3, p. 445, particularly the areas reported for silica gel based on water vapor adsorption. These figures, averaging 76 sq m per g, are about 15 percent of the probably correct value.

In Table A, we show results of water vapor adsorption measurements made here several years ago on a sample of silica gel prepared in three different ways. Indicated specific surfaces range from 505 to 634 sq m per g. These are about the same as the values usually obtained by nitrogen adsorption and are not far from the nitrogen figure obtained by Gleysteen and Kalousek.

Our data were obtained by means of the air-stream apparatus described in *Portland Cement Assn. Bulletin* No. 22, p. 269. The samples thus were exposed to streams of humidified air in a system free from CO_2.

Using data obtained by Ewing and Bauer (*Journal of American Chemical Society*, V. 59, 1937, p. 1548), we found water vapor adsorption to give a specific surface of 490 sq m per g, which, although smaller than figures shown in Table A, are much larger than the figures found by Gleysteen and Kalousek. The gel used by Ewing and Bauer contained about 4.6 g of water per 100 g of dry gel at the start of the test, this amount of water being the lowest that could be produced by storing specimens over concentrated sulfuric acid in a vacuum.

Recent ideas on the structure of silica surfaces suggest an explanation for the low surface area obtained by Gleysteen and Kalousek. Ordinarily, the surface of silica is covered with hydroxyl groups; this is called "silanol" surface. The surface can be dehydrated, partly or completely, by heating or by other means; this is called "siloxane" surface. The silanol surface adsorbs

*ACI JOURNAL, Jan. 1955, *Proc.* V. 51, p. 437. Disc. 51-22 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

†Portland Cement Assn., Chicago, Ill.

TABLE A—SPECIFIC SURFACE OF SILICA GEL BY WATER ADSORPTION

Sample No.	Method of drying	Days allowed for adsorption	Surface area, sq m per g
1	5 hr at 200 C in vacuum oven, then over P_2O_5 for 2 days at 23 C	29	505
2	Overnight in vacuum oven at 204 C	30	634
3	Dried to constant weight over P_2O_5 at 23 C	33	566

water readily, the siloxane surface with difficulty (Iler, *The Colloid Chemistry of Silica and Silicates*, Cornell University Press, 1955, p. 234). The siloxane surface acts like a hydrophobic material; it gives a Type III adsorption isotherm (Brunauer, *The Adsorption of Gases and Vapors*, Princeton University Press, 1943, p. 150). At the relative humidity used by Gleysteen and Kalousek, 0.15, only a fraction of a siloxane surface would be covered with water; on hydrophobic surfaces complete covering is obtained only at relative humidities of 0.5 or greater.

Iler points out that the siloxane surface is slowly rehydrated upon exposure to water. This explains the slow equilibration of water vapor over silica gel, shown in Table A.

It seems clear from the above considerations that the results obtained by the proposed rapid technique are not correct for silica gel. The discrepancies between the nitrogen and water surface areas shown in Fig. 3 indicate that the rapid technique gave incorrect results also for the other items listed, with the exception of the first two. The explanation may be the same as that given above for silica gel; *i.e.*, the materials had hydrophobic surfaces.

The source of error appears to us to be in one or both of two critical parts of the technique: the initial "outgassing" (drying) period may be too brief, and the 24-hr period of exposure to water vapor is almost certainly inadequate.

With respect to the second factor, length of time required for reaching equilibrium with a given vapor pressure, see Fig. A. This shows water uptake by a sample of silica gel, in relation to elapsed time, for samples exposed to a stream of air maintained at 0.158 relative humidity. Notice that adsorption continued during the whole 40-day period of observation. Of the total amount of water adsorbed in this period, about 10 percent was taken up during the first 24 hr. Since Gleysteen and Kalousek allowed only a 24-hr exposure, it seems likely that the amount of water adsorbed in that time was a similar fraction of the total capacity of the sample. If the correct surface is 500 sq m per g, the indicated fraction is 15 percent. The fraction adsorbed in evacuated apparatus during the first 24 hr would be expected to be higher than that adsorbed in the air-stream apparatus. Ewing and Bauer, who used evacuated apparatus, reported that 3 or 4 days were required for equilibrium. We surmise that even 3 or 4 days were inadequate for surface area measurements.

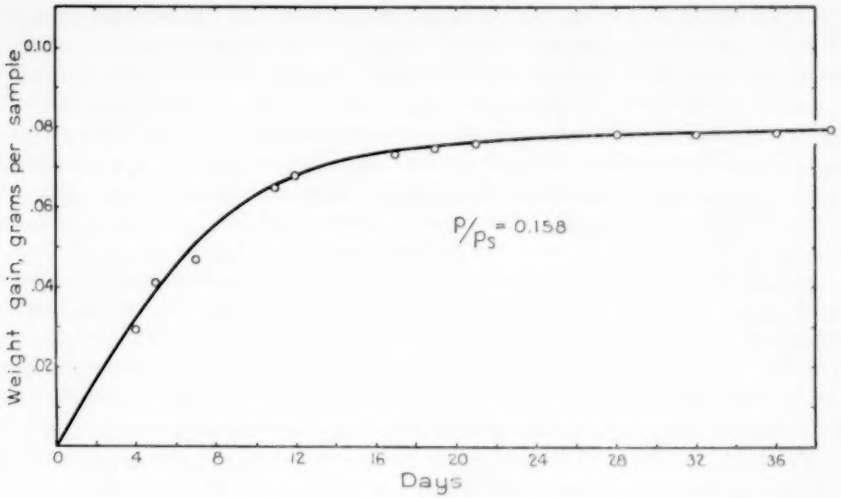

Fig. A—Water vapor adsorption by silica gel

Although results reported by Gleysteen and Kalousek for portland cement paste, Table 2, p. 443, are more reasonable than those in Table 3, it seems likely that there is a wide margin of uncertainty. We do not think it possible to prepare all kinds of specimens properly, using a room temperature procedure, within an arbitrary 3-day period, and we believe the 24-hr period of adsorption much too short, when using water vapor.

In our work, all samples are dried in a vacuum desiccator 5 days or more before they are used for adsorption measurement. The time is always that required to remove water having vapor pressure in excess of 5×10^{-4} mm of Hg. Small variations in amounts of residual water in excess of the standard state of dryness produces significant variations in indicated specific surface.

If an arbitrary 3-day outgassing period is used, as recommended by the authors, most cement paste samples are liable to contain some evaporable water at the end of the period. The amounts retained will differ according to differences in permeability. Therefore, it is questionable to use a fixed, short outgassing period on the assumption that if all samples are treated equally, results will be comparable.

It is true that the authors' Table 1, p. 440, indicates that results were comparable. But in Table 2, twofold variations in specific surface among samples that ought to be alike seem to throw doubt on the indications of Table 1. In our work we find that portland cement pastes made from the same cement and hydrated to the same degree show only a few percent variation in water vapor adsorption per unit of cement in the sample.

Experience has taught us that water vapor adsorption results are not reliable unless time is taken for proper preparation of samples and for a reasonably close approach to adsorption equilibrium.

From previous correspondence with Dr. Kalousek, we understand that the block cured in high-pressure steam contained a certain quantity of silica flour, perhaps 30-40 percent of the cement weight. In such case, the silica flour reactions at high temperature might account for the unexpectedly high surface areas found by nitrogen adsorption. Curing at 340 F (which corresponds to 120 psi steam pressure) for 8 hr would cause silica flour and calcium hydroxide liberated by portland cement to form a calcium silicate hydrate. Probably this product has high specific surface at first, though prolonged steaming will convert it to a low-surface, noncolloidal state.

In an earlier paper, Kalousek drew attention to a parallelism between interlayer water and drying shrinkage ("Fundamental Factors in the Drying Shrinkage of Concrete Block," ACI JOURNAL, Nov. 1954, *Proc.* V. 51, pp. 233-248). In that paper only averages obtained for the six different concretes were given, but the present paper gives figures for the concretes individually in Table 2. Our calculations, based on Table 2, are given in Table B.

Kalousek compared the averages of the last two columns shown in Table B, and found that they were in the proportion of 7 to 6. He directed attention to this agreement because it suggested the possibility that the amount of interlayer water may be proportional to shrinkage but indicated that no definite conclusion could be drawn without further experimentation.

We believe that the data, when examined in detail, indicate no such proportionality. Interlayer water shows a tenfold variation from highest to lowest, whereas shrinkage changes only by 44 percent. The interlayer water ratio for sand and gravel concrete is more than three times as large as the shrinkage ratio, whereas for expanded shale the opposite is indicated: the shrinkage ratio is between two and three times as large as the interlayer water ratio. The mean of interlayer water ratios is 2.2, but the standard deviation of the mean is 2.4, indicating that uncertainty in any of the values is about as great as the value itself.

It is possible that Bernal's suggestion as to the role of interlayer water in shrinkage of concrete will, in the long run, prove to be correct. However, we believe that only the most painstaking experimentation will be able to prove or disprove the hypothesis.

TABLE B—RELATIONSHIP BETWEEN INTERLAYER WATER AND DRYING SHRINKAGE

Aggregate	Area of interlayer water, sq m per g		Ratio	Shrinkage ratio
	NC	HP	NC/HP	NC/HP
Sand and gravel	49	7	7.0	2.3
Cinder	55	40	1.4	1.8
Expanded shale	36	51	0.7	1.6
Expanded slag	46	27	1.7	1.7
Sintered shale	87	70	1.2	1.6
Pumice	40	29	1.4	1.6
Average			2.2	1.8

As for using a single adsorption point, we believe that is feasible when relative areas will suffice. It seems, however, that the proposed method would be improved if a different assumption were made about constant c of the equation on p. 441. Rather than assuming that c equals infinity, which is done when a line is drawn from the experimental adsorption point to the origin of the BET plot, we would prefer to draw the line to a point on the negative x axis corresponding to an assumed finite value for c. This method of plotting is discussed on p. 480 and is illustrated in Fig. 3-5, p. 481, of *Portland Cement Assn. Bulletin* No. 22.

Before applying the one-point method to a particular kind of material, the experimenter should know the average value of c for that material. For silica gel we find c to be about 8; for cement pastes, the value is considerably higher, about 35.

Although the one-point method may be useful for quality control in industrial practice, we advise against using it for research purposes.

AUTHORS' CLOSURE

It seems necessary to restate once again the purpose of the adsorption studies under discussion and the interpretations[1] based on these to reply to the criticisms of Powers, Brunauer, and Copeland. The original project was set up to devise a *rapid* physicochemical method to predict the shrinkage-swelling phenomena of autoclaved and low-pressure steam-cured concrete block. Commerically produced block were used in the tests. Accordingly, it would be expected that errors due to sampling would be high. However, this alone was not considered serious because it was expected, on the basis of earlier theories, that the low-pressure steam-cured block would have surface areas many times larger than those of autoclaved block. Therefore, the use of a method of low precision seemed admissible if time could be saved in completing a test. In many fields of applied science it becomes necessary to strike a balance between precision and rapidity.

Although the ostensible purpose of the study was not realized, for the basic reason that the old theories were incorrect, the data appeared suitable for interpreting the drying shrinkage phenomena of concrete. The fact that the precision of the results was not all that could be desired did not seem reason enough to withhold publication of the interpretations. Effort was made to ascertain and to present the limitations of the data as these applied to concrete block.

Regarding the criticisms of Powers *et al* on the times allowed for degassing and exposing the concrete block samples to water vapor, we were fully aware at all times of the necessity of obtaining results either under equilibrium conditions or under conditions, clearly defined, but somewhat removed from equilibrium. The results given in Table 1 and others not given but discussed in the text (pp. 440-441) were carried out explicitly for this reason. It may be added that three operators (Gleysteen, Brady, and Logiudice) checked and rechecked the results. Reference to Table 1, p. 440, shows that no

additional water was adsorbed by the concrete block specimens on the fourth day of exposure to water vapor. Except for the sand and gravel block, which we shall show to be basically different from the other specimens, the amount of water adsorbed in 24 hr was nearly a constant percentage of that adsorbed in a 96-hr period. The results observed by the third operator were obtained on samples kept in dry storage for a year. These results were found to differ by 3 to 15 percent from those obtained initially by the first two operators. The precision, of course, is not high but for reasons already stated, it was considered satisfactory. However, deviations of the order of 15 percent are much higher than expected, and a possible explanation for these will be advanced later in this discussion.

In view of the results and discussion, the basis used by Powers *et al* for questioning the validity of our results is most surprising. They state that the results on block of different aggregate (Table 2) "Ought to be the same" because ". . . portland cement pastes made from the same cement show only a few percent variation . . ." Now, from a consideration of the chemistry of cement-pozzolan reactions one could surmise the opposite, that is, the results for different aggregates *might be different.* Direct experimental results show that *they are different.*

Shortly after the adsorption data were gathered, tests were carried out to substantiate the hypothesis based on the chemistry of portland-pozzolan reactions as follows. The block were crushed gently and as much of the aggregate as possible was removed by hand. The remainder was crushed further to separate the paste from the larger pieces of aggregate and then sieved through a No. 50 screen. These fines were subjected to differential thermal analysis. The results revealed that the block made of soft aggregates and cured with low-pressure steam (3 hr up to 170 F, 1 hr at 170 F, and 12 hr coasting to 125 F) did not contain $Ca(OH)_2$. This indicates that the lime-pozzolan reaction may occur relatively rapidly under the comparatively mild conditions of steam curing. The DTA results also suggested that the nature of the binders among these block might vary. However, aggregate fines in the samples, in unknown amounts, made the analysis of x-ray data difficult. Also, $Ca(OH)_2$ could have been absent simply because it had combined with the CO_2 of the air. For these reasons, pastes were prepared using 60 percent cement and 40 percent of fines of each one of the aggregates at a water to solids ratio of 0.75 by weight. These pastes were cured in closed containers to exclude CO_2 following the above given cycle, and dried at 100 C. The samples were ground and examined by x-ray diffraction. The binder of the silica sand product appeared to differ fundamentally to a large degree from that of the other specimens. The pattern was highly complex, containing complete diffraction lines of quartz and $Ca(OH)_2$. The strong lines of both the alpha-type, and beta-type (hillebrandite) dicalcium silicate hydrates were also present. Moreover, DTA results indicated that the gel-like, lime-rich phase forming at room temperature was also present. Whether this binder was actually a mixture of all these phases, or of fewer, still un-

identified products, could not be definitely ascertained. The binder of the other pastes consisted entirely or largely of the hydrous calcium silicate forming at room temperature. The paste made with the slag binder also contained free $Ca(OH)_2$ in moderate amount; the others did not, or only in small amount. Because the reactivity of different aggregates is different and the amounts of Al_2O_3 and Fe_2O_3, which presumably enter the lattice,[2] also varied among the aggregates, it follows that differences, at least in physical properties, would occur among these.

In view of the results in Table 1, the discussions, and the chemistry of the pozzolan-cement reaction, it seems that the assumption of Powers *et al* is not valid. Accordingly, we cannot accept their criticism of the block data on this basis.

The criticism by Powers *et al* regarding the use of average data for computing the amount of "interlayer" water is fully justified. It must be made clear, however, that this result was used for illustrative purposes and not as a basis for the development of the theory on shrinkage. In fact, these data were presented as an afterthought and this is the reason why the oversight was made of not checking the individual results. The results were submitted, with a word of caution, merely as a suggestion for future research. Accordingly, the reference by Powers *et al* to Bernal's suggestion seems much out of place, because the reader is left under the impression that the computed amounts of interlayer water were indeed a basis for the theory. As a matter of fact, quite the opposite is correct; an attempt was made to explain experimental data under discussion on basis of the new theory.

Since Powers *et al* have raised the question of the relation between calculated amount of interlayer water and shrinkage by reproducing the data in their Table B, it is well to point out that the individual data in that table are probably not as inapplicable for the intended purpose as they may seem. In the first place, and for reasons already stated, the result on the sand-gravel block would have to be deleted because in this case the binder is fundamentally different, at least in large part, from that of the other block. Out of the remaining five block, only that made of shale is an exception to the average. The real reason for this exception is not evident, but a re-examination of the texture and physical appearance of the block indicated that the low-pressure shale block might have been taken from a batch poorer in cement than that which was autoclaved. Admittedly, in view of the limitations and the one uncertainty, the data cannot be used for drawing any conclusion. However, because of the difficulties in the way of proving the mechanism of shrinkage, it seems that measurements and computations such as those discussed would make a useful basis for attacking the problem. In such an investigation, commercially prepared block should not be used.

There is a great deal of confusion regarding use of the terms "surface area by water vapor adsorption" and "evaporable water." The very large surface "areas" of cement pastes obtained by vapor adsorption are not true areas. Water apparently enters the lattice of some of the hydrous calcium silicates,

perhaps similarly, as it does in zeolites or certain clays and other minerals to be mentioned. How much of the total water is adsorbed in the lattice is not known. The areas computed from total amounts are, therefore, fictitious. In fact, for results obtained under equilibrium conditions, one could obtain any number of reasonable but significantly different values for surface areas by altering the conditions of preparing the sample for test.

We used the term "apparent surface area," but even this may be misleading. It would be better merely to speak of amounts of water adsorbed per unit weight. In our studies, the amount of water vapor adsorbed had to be expressed in area units to permit comparison of the results obtained by the two methods used.

Cement pastes, hydrous calcium silicates, certain clays, and zeolites are not the only solids to adsorb more water than required to cover the true surface. Recently Young and Healey[3] in highly refined measurements reported surface areas of 9.6 to 9.9 sq m per g for chrysotile using N_2, A, CO, C_2H_2, C_4H_{10}, $(CH_3)_3N$, and $(CH_3)_2NH$. Water and NH_3 showed apparent surface of 17.6 sq m per g, nearly double that of the other adsorbates. These authors believed that the polar substances, water and NH_3, entered pores of the cylindrical fibers of that mineral, and that the nonpolar adsorbates did not. However, anamalous results of N_2 and water adsorption were also obtained on antigorite, which mineral is crystallographically the same as chrysotile except that it exists in lamellar plates, not fibers. For this and other reasons, it was suggested[4] that the water (and NH_3) in significant amount was accommodated differently than the other adsorbates, probably in the lattice. Since tobermorite, which is the binder of autoclaved block, crystallizes as flat plates and shows about the same ratio of water to nitrogen adsorption (about 2 to 1) as does chrysotile, it follows that some of the water adsorbed is also accommodated differently than is the N_2. The exact manner in which that water is held in the lattice is still unknown.

The term "evaporable water" is misleading because the water removed by in-vacuum drying includes some lattice water. The lower the vapor pressure, at a fixed temperature, or the higher the temperature at a fixed vapor pressure, the greater is the amount of water desorbed. If desorption and adsorption are completely reversible, it follows that the amount of water adsorbed would depend on the processing of the sample for test. If calcium sulfoaluminates are present in the cement paste, and undoubtedly they are present at least during curing periods of several days to several weeks, these are dehydrated to a large degree by in-vacuum drying at ordinary temperature. Accordingly, evaporable water would include free water, surface adsorbed water, and hydrate water. If the drying is done at even moderately elevated temperatures, some hydroxyl water is also probably lost. No method is known by which the present samples could be stripped free of surface adsorbed water with the amount of lattice water remaining constant. The desorption of two differently held waters probably occurs simultaneously but in different ratios with changing drying conditions. For this reason,

the conditions for drying are entirely arbitrary. However, to obtain reproducible results, even though these may be meaningless for estimating the surface area, it is absolutely necessary to attain equilibrium conditions for precise measurements of adsorption. The amount of water adsorbed, if the true surface is known, can be highly valuable for qualitative interpretation of the nature of the cement paste. In some cases, even quantitative estimates could be made of the effects of firmly held water in the paste on the properties of concrete. In making qualitative interpretations, small departures from equilibrium conditions would be inconsequential.

In regard to comparison of the rate of water adsorption by concrete block and silica gel, it should be noted that concrete block, either autoclaved or low-pressure steam cured and stored in the laboratory air as in our tests, contains relatively coarse pores. On the other hand silica gel is known to be characterized by extremely fine pores. For this reason, the time required to attain equilibrium differs for these two solids. As pointed out by Powers *et al*, attainment of equilibrium with silica gel is slow and the apparent surface area available to water vapor in the time allowed in our tests was very low. Prolonged exposure is required to attain equilibrium and, therefore, the true area. The authors did not make this point clear and are obliged to Powers *et al* for directing attention to it. However, it may be well to note that the time required to attain equilibrium in an air flow system need not be precisely indicative of the time required in an air-free vapor system.

Another limitation of the apparent surface area results obtained by water vapor adsorption is indicated by unpublished results obtained in this laboratory. Samples of certain hydrous calcium silicates undergo significant changes in surface areas during dry storage. An unidentified editorial critic of the paper on the theory of shrinkage of concrete block,[1] in commenting on the manuscript while it was being processed for publication, stated that he had observed changes in the surface areas of cement pastes in dry storage. These results are both challenging and perplexing. The experimenter obtaining them naturally is reluctant to publish for fear of being adversely criticized for using poor experimental techniques. It was speculated that a change of apparent surface area during storage could possibly result from a recrystallization of the crystalline solids comprising the binder. To probe this hypothesis, several different hydrous calcium silicates were prepared. These were dried in vacuum, rehumidified, and finally examined by x-ray diffraction. The technique of test and some of the data have been published.[5] These results show that in-vacuum drying and rehumidification apparently causes an internal recrystallization of some, but not all, of the solids examined. In further tests small bars were studied similarly, and also measured for volume change. The results, which were presented by Kalousek before the research session of the American Concrete Institute convention in 1955, showed that internal recrystallization was associated with the volume changes of the specimens. At the time, facilities were not available for surface area measurements, but it may be inferred that the lattice capacity for water

was altered by the in-vacuum drying of some of the solids. The difference of 15 percent in water vapor adsorption, by the concrete block samples in duplicate tests mentioned earlier in this discussion, could have been due to some actual change such as recrystallization rather than to experimental error.

Regarding the use of water vapor adsorption, as either a research tool or for tests of quality, it is evident that the techniques have to be resolved for each particular application. The conditions of degassing, especially in tests of substances containing lattice water subject to in-vacuum desorption, have to be carefully established and defined. In view of our experience, and that of others, the time requirements for exposure to the water vapor may range from a few days, as happened to be the case with the concrete block of this study, to several months. Tests on cement pastes showed that both the time of degassing and adsorption were longer than for the concrete block. Apparently the physical nature of the solids has an important effect on the rate at which the water is lost and adsorbed. The choice of techniques of water vapor adsorption, whether for research or control, will depend on the precision required of the result. If the precision required is not high, as would have been the case in the attempted comparison of autoclaved and low-pressure steam cured block, had the old theories applied, then the simplest method would have served the purpose.

The writers do not agree with the arbitrary view of Powers *et al* that the one-point method may not be suitable for research. As already mentioned, the required precision of the data is the criterion on which the choice of method will depend.

For reasons given in this discussion, it appears that water vapor adsorption is a powerful research tool for studying concrete and related solids. It is not, however, an accepted method for measuring surface areas of many solids, and this includes portland cement pastes.

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Discussion of a paper by Milo S. Ketchum:

Design and Construction of a Folded Plate Roof Structure*

By JAMES CHINN†

The design of longitudinal reinforcement for the H. W. Moore Equipment Co. building differed from the method of Messrs. Winter and Pei‡ where each plate is designed by the conventional concrete theory for a member subject to direct stress and bending.

Instead, enough steel was provided that the moment about the neutral axis of the steel area times the allowable steel stress was equal to the moment about the neutral axis of the tension stress block if the plate were a homogeneous material.

In the latter stages of the design, a method of analysis was developed which gave edge stresses in a homogeneous plate directly without solving for the interaction forces between adjacent plates. This method was later found to be identical with that used by Prof. Gaafar,§ but a further explanation of the derivation might be of interest.

Consider a transverse section of any plate, n, Fig. A, subjected to M_0, the moment at the section due to external loads; to N_T, the edge shear force acting upon the section due to interaction with plate $n + 1$; and to N_B, the edge shear force acting upon the section due to interaction with plate $n - 1$.

Considering f_B in tension and f_T in compression to be positive, the stresses at the top and bottom of the plate due to M_0 are:

$$(f_T)_{M_0} = (f_B)_{M_0} = \frac{M_0}{S} \text{ where } S = \text{section modulus} = \frac{bh^2}{6} \quad \ldots\ldots (1)$$

The stresses due to N_T and N_B are:

$$(f_T)_{N_T} = -\frac{N_T}{A} - \frac{N_T(h/2)(6)}{bh^2} = -\frac{N_T}{A} - \frac{3N_T}{A} = -\frac{4N_T}{A} \quad \ldots\ldots (2)$$

$$(f_B)_{N_T} = \frac{N_T}{A} - \frac{3N_T}{A} = -\frac{2N_T}{A} \quad \ldots\ldots (3)$$

$$(f_T)_{N_B} = -\frac{2N_B}{A} \quad \ldots\ldots (4)$$

$$(f_B)_{N_B} = -\frac{4N_B}{A} \quad \ldots\ldots (5)$$

*ACI JOURNAL, Jan. 1955, *Proc.* V. 51, p. 449. Disc. 51-23 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

†Assistant Professor of Civil Engineering, University of Texas, Austin (on leave).

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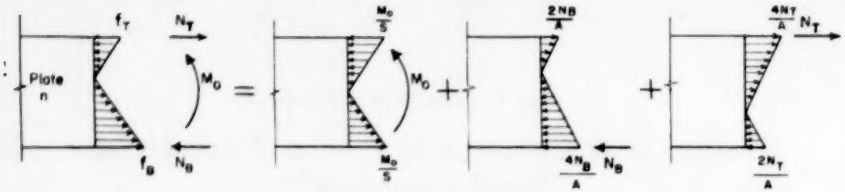
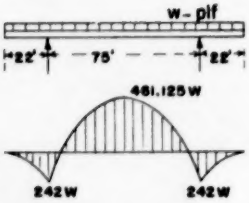
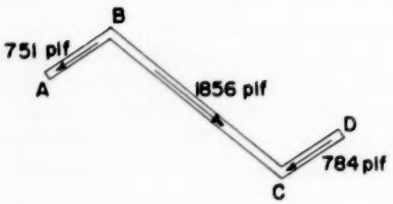


Fig. A—Forces on transverse section of plate



M_o for all plates



Forces resolved into planes of plates

FOR STRESSES AT SUPPORTS

Slab	Area	S - in³	"K" = $\frac{I}{A}$	M_o - kf	$\frac{M_o}{S}$ - psi
AB	5.33 X 0.33 = 1.78	2,730	0.562	-181.7	-797
BC	12.04 X 0.33 = 4.01	13,920	0.2495	449.2	388
CD	5.18 X 0.33 = 1.73	2,580	0.578	-189.7	-882

	A	B			C		D
"DF"	0	0.692	0.308		0.302	0.698	0
"FEM"	-797	-797	388		388	-882	-882
			74	←	149	345	→ 172
	116	← 232	103	→	52		
			-8	←	-16	-36	→ -18
	3	← 6	2	→	1		
						-1	
Stresses	-678	-559	559		574	-574	-728
	Compr.	Tens.			Compr.		Tens.

Fig. B—Calculations for stresses in Z-slab

Then the total stresses are:

$$f_B = \frac{M_0}{S} - 4N_B(1/A) - 2N_T(1/A) \quad (6)$$

$$f_T = \frac{M_0}{S} - 4N_T(1/A) - 2N_B(1/A) \quad (7)$$

At edge n, $(f_T)_n$ and $(f_B)_{n+1}$ are the same stress, but since $+ f_B$ is tension and $+ f_T$ is compression:

$$(f_T)_n = -(f_B)_{n+1} \text{ or } (f_T)_n + (f_B)_{n+1} = 0 \quad (8)$$

Eq. (6), (7), and (8) correspond to the slope-deflection equations

$$M_{AB} = M^F_{AB} - 4\Theta_A\left(\frac{EI}{L}\right) - 2\Theta_B\left(\frac{EI}{L}\right) \quad (9)$$

$$M_{BA} = M^F_{BA} - 4\Theta_B\left(\frac{EI}{L}\right) - 2\Theta_A\left(\frac{EI}{L}\right) \quad (10)$$

At joint B, $M_{BA} + M_{BC} = 0$ (11)

Since M_{AB} and M_{BA} may be found by moment distribution without first solving for Θ's, an analogous moment distribution will give f_B and f_T without first solving for N's. Analogous terms are:

Slope deflection	Stress distribution
M_{AB}	f_B
M_{BA}	f_T
M^F_{AB}	M_0/S
M^F_{BA}	M_0/S
Θ_A	N_B
Θ_B	N_T
EI/L	$1/A$

Calculations for stresses in the z-slabs are as given in Fig. B.

Discussion of a paper by Mario G. Salvadori and A. D. Ateshoglou:

Ribless Cylindrical Shells*

By H. S. GEDIZLI and BENDT WEDELL

ERRATA

The following equations and tabular values should be substituted for the corresponding equations and tabular values in the paper.

These changes are on pp. 459 and 460; they were suggested by B. Wedell and checked by the authors.

The changes in Eq. (9) do not affect the corresponding tabular values.

$$M_d = \frac{1}{\pi^2/2 - 4}\left[\frac{\pi}{2} - (\pi^2/2 - 4)\,\phi \sin\phi + (\pi^2/4 - 4)\cos\phi\right] p_o r^2 \quad (9)$$

$$M_w = \frac{1}{\pi}\,(\phi\cos\phi - \tfrac{1}{2}\sin\phi)\,p_w r^2 \quad (10)$$

$$M_T = \frac{1}{6(\pi^2/2 - 4)}\,(2 - \pi\cos\phi)\,\alpha\, TEt^3/r \quad (11)$$

$$M_T = \frac{1}{3\pi}\,(\cos\phi)\,\alpha\, TEt^3/r \quad (15)$$

TABLE 1—FIXED SHELL

	V_a/pr	H_a/pr	M_a/pr^2	M_c/pr^2	M_{max}/pr^2	M_{min}/pr^2	
						Value	Location ϕ, deg
p_c	±0.159	∓0.500	∓0.159	0	±0.159	±0.070	37
	$V_{aT}r^2/\alpha TEt^3$	$H_{aT}r^2/\alpha TEt^3$	$M_{aT}/\alpha TEt^3$	$M_{cT}/\alpha TEt^3$	$M_{maxT}/\alpha TEt^3$	$M_{minT}/\alpha TEt^3$	
						Value	Location ϕ, deg
$\pm T$	0	±0.569	±0.356	∓0.204	±0.356	∓0.204	0, 90

TABLE 2—HINGED SHELL

	$V_{aT}r^2/\alpha TEt^3$	$H_{aT}r^2/\alpha TEt^3$	$M_{aT}/\alpha TEt^3$	$M_{cT}/\alpha TEt^3$	$M_{maxT}/\alpha TEt^3$	$M_{minT}/\alpha TEt^3$	
						Value	Location ϕ, deg
$\pm T$	0	±0.106	∓0.106	∓0.106	±0.106	∓0.106	90

*ACI JOURNAL, Jan. 1955, *Proc.* V. 51, p. 457. Disc. 51-24 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

By H. S. GEDIZLI*

The paper deals with the integration of some analytical expressions. The proposed one-dimensional method of analysis is well known and affords the best approximation for longer shells.[1] The authors, however, do not mention the criterion necessary to decide beforehand whether the shell is sufficiently long to allow use of the method. The necessary criterion, which will supplement the paper, was given by J. Moe.[2]

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2. Moe, J., "On the Theory of Cylindrical Shells. Explicit Solution of the Characteristic Equation and Discussion of the Accuracy of Various Shell Theories," *Publications*, International Assn. for Bridge and Structural Engineering (Zurich), V. 13, 1953, pp. 283-296.

By BENDT WEDELL†

This discussion presents moment diagrams for ribless cylindrical shells, either fixed or hinged at the edges, subject to five different loading cases. Only four different loading cases were discussed by the authors. The notation used here is the same as in the paper.

A 1 ft wide section of the cylindrical shell is considered cut out and acting as an arch subject to forces and moments acting in the plane parallel to the arch only. This assumption is all right for shells having no end or intermediate supporting walls (or ribs), or if this is not the case, it will be a sufficiently accurate assumption for that part of the shell which is located more than about $0.5r$ from such support. Assume shell supported on nonyielding foundations and stresses in shell are below critical buckling stress.

It is beyond the scope of this discussion to determine the critical buckling stress. It is, however, important—especially for thin mono-curvature shells having no end walls or similar stiffening members—to make sure that the critical stress is not exceeded at any point.

Fixed shell (Fig. A)

The moment diagrams shown are based on Eq. (8) of the paper and Eq. (9), (10), and (11) as corrected in the errata. These can be derived by column analogy. The additional case covering earthquake load is

$$M_Q = [\phi \cos \phi - \tfrac{1}{2} \sin \phi]\, cp_d r^2$$

M_Q is the moment due to earthquake load cp_d, where p_d is the unit dead load of the shell, c is the earthquake "factor" = lateral force/dead weight, r the shell radius, and ϕ is the angle between the vertical line and a radius to the point considered. For earthquake loads, the formula is derived assuming forces from the left toward the right. As in the paper, M_d is moment due to dead load, M_u moment due to uniform distributed live load, M_w

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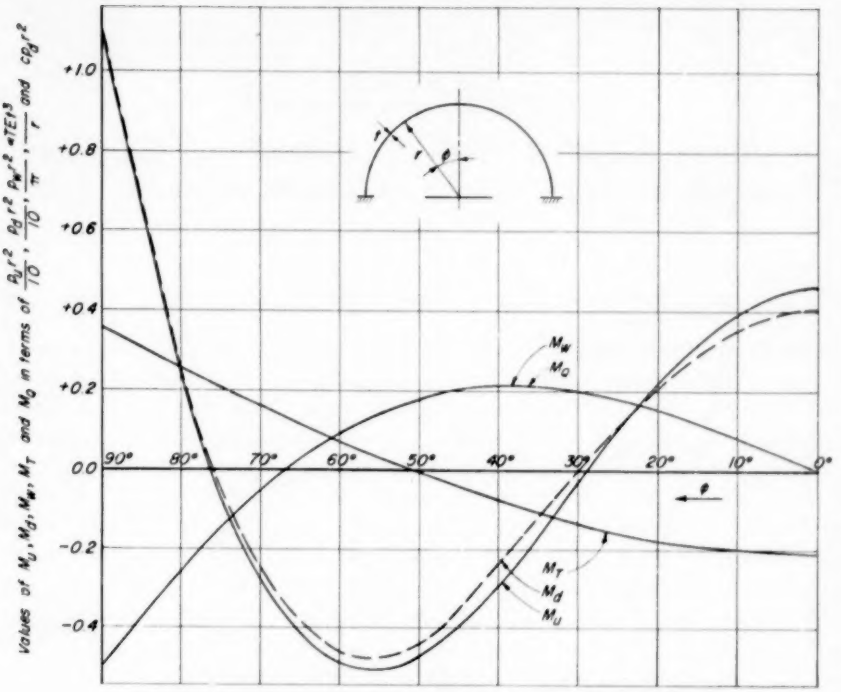

Fig. A—Moment diagrams for fixed shell—five loading conditions

moment due to wind load, and M_T moment due to temperature variation (T positive for increase in temperature).

Hinged shell (Fig. B)

In addition to the equations for M_d, M_u, M_w, and M_T, the moment diagram due to earthquake loading is

$$M_Q = [\phi \cos \phi]\, cp_d r^2$$

The moment curves shown on the diagrams are for the left half of the shell. Curves M_u, M_d, and M_T are symmetrical about the centerline of shell; curves M_w and M_Q are antisymmetrical about centerline of shell.

From the moment curves can be derived:

Dead load moment $M_d = \dfrac{p_d r^2}{10} y_d$

where y_d is the ordinate in the M_d curve.

Live load moment $M_u = \dfrac{p_u r^2}{10} y_u$

where y_u is the ordinate in the M_u curve.

Wind moment $M_w = \dfrac{p_w r^2}{\pi} y_w$

where y_w is the ordinate in the M_w curve.

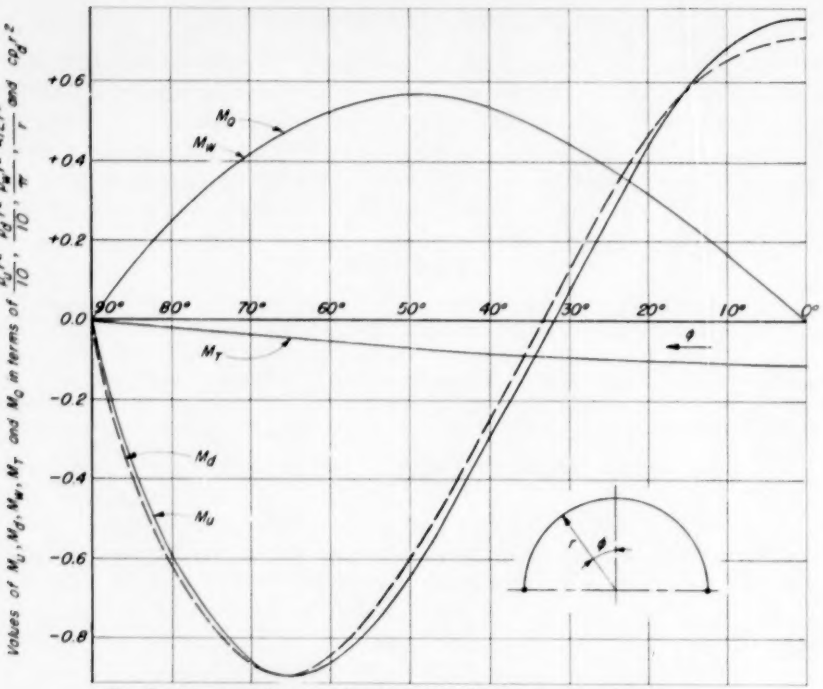

Fig. B—Moment diagrams for hinged shell—five loading conditions

Temperature moment $M_T = \dfrac{\alpha\, TEt^3}{r}\, y_T$

where y_T is the ordinate in the M_T curve.

Earthquake moment $M_Q = cp_d r^2\, y_Q$

where y_Q is the ordinate in the M_Q curve.

Tables A and B supplement Tables 1 and 2 with values for earthquake loading.

TABLE A—FIXED SHELL

Load	V_a/cpr	H_a/cpr	M_a/cpr^2	M_c/cpr^2	M_{max}/cpr^2	M_{min}/cpr^2	
						Value	Location ϕ, deg
cp_d	0.500	−1.571	−0.500	0	0.500	−0.500	90

TABLE B—HINGED SHELL

Load	Va/cpr	H_a/cpr	M_c/cpr^2	M_{max}/cpr^2	M_{min}/cpr^2	
					Value	Location ϕ, deg
cp_d	−1.000	−1.571	0	0.560	−0.560	−49

Discussion of a paper by R. E. Philleo:

Comparison of Results of Three Methods for Determining Young's Modulus of Elasticity of Concrete*

By BRYANT MATHER and AUTHOR

By BRYANT MATHER†

The information compiled and ably discussed by Mr. Philleo will be of material assistance to others who are confronted with the necessity of comparing values for Young's modulus of elasticity of concrete obtained by different testing procedures. As the author noted, there is no generally accepted basis for computing the static value of Young's modulus. The Corps of Engineers has, however, standardized a procedure for use in connection with its work. This procedure, designated CRD-C 19,[1] involves computation of the chord modulus from strain readings at 250 and 1000 psi.

In connection with investigational work at this laboratory, tests were made on specimens representing 193 different concrete mixtures, 109 at a water-cement ratio of 0.8 by weight and 84 at a ratio of 0.5. Each mixture was represented by three batches of concrete made on different dates. At an age of 180 days, one 3 x 6-in. cylinder was tested for static modulus by CRD-C 19 and two $3\frac{1}{2}$ x $4\frac{1}{2}$ x 16-in. beams were tested for fundamental transverse frequency by ASTM Designation C 215.[2] Fig. A shows the average values for each of the 193 mixtures. It will be noted that the dynamic moduli are about 1×10^6 psi higher than the corresponding static moduli. It would be interesting to know whether this difference is primarily the result of the difference in size and shape of the specimens used, the particular sort of static modulus computed, or both; and whether such a relation exists at other ages.

REFERENCES

1. *Handbook for Concrete and Cement* (with quarterly supplements), Corps of Engineers, Waterways Experiment Station, Vicksburg, Miss., 1949.

2. *Book of Standards*, Part 3, American Society for Testing Materials, Philadelphia, Pa., 1952.

AUTHOR'S CLOSURE

In comparing the static modulus of cylinders with the dynamic modulus of beams, Mr. Mather introduces a pertinent aspect of the subject which was

*ACI JOURNAL, Jan. 1955, *Proc.* V. 51, p. 461. Disc. 51-25 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

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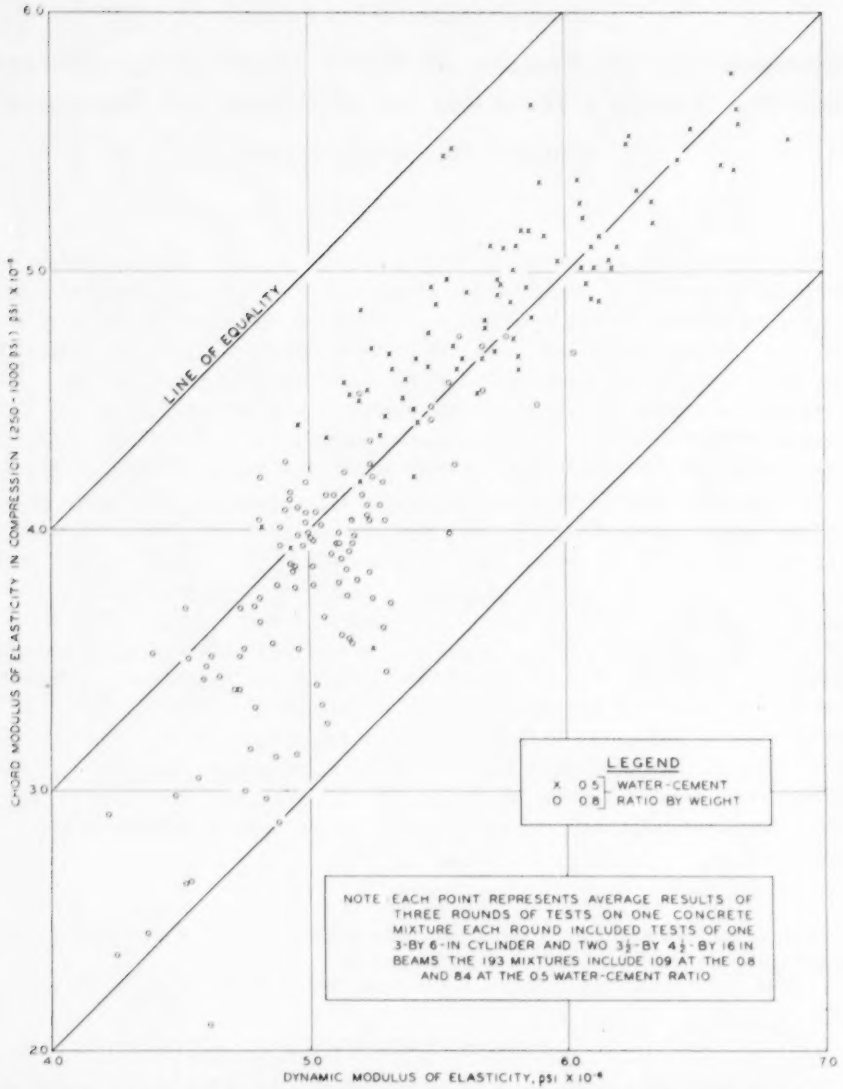
LINE OF EQUALITY
LEGEND
x 0.5 WATER-CEMENT
o 0.8 RATIO BY WEIGHT
NOTE: EACH POINT REPRESENTS AVERAGE RESULTS OF THREE ROUNDS OF TESTS ON ONE CONCRETE MIXTURE. EACH ROUND INCLUDED TESTS OF ONE 3-BY 6-IN CYLINDER AND TWO 3½-BY 4½-BY 16 IN BEAMS. THE 193 MIXTURES INCLUDE 109 AT THE 0.8 AND 84 AT THE 0.5 WATER-CEMENT RATIO
CHORD MODULUS OF ELASTICITY IN COMPRESSION (250 - 1000 psi) psi x 10⁻⁶
DYNAMIC MODULUS OF ELASTICITY, psi x 10⁻⁶
2.0
3.0
4.0
5.0
6.0
4.0
5.0
6.0
7.0

Fig. A—Dynamic moduli of beams are approximately 1 × 10⁶ psi higher than chord moduli of cylinders from the same batches of concrete at 180 days

omitted from the original paper, namely, the effect of size and shape of specimen. In the work reported in the paper, each comparison was between results of the different types of tests on the same specimen. Mr. Mather's results appear at first glance to be at odds with the data in Tables 1 and 2. However, the cylinder static modulus shown in the table is the initial tangent modulus, whereas Mr. Mather reports chord modulus between 250 and 1000 psi. For the cylinders in Table 1 the secant modulus at 25 percent ultimate strength was also computed. This modulus is somewhat comparable to the one used by the Corps of Engineers and is considerably lower than the initial tangent modulus. For as nearly a direct comparison with the Mather data as is possible, the dynamic modulus of the beams in Table 1 exceeded the secant modulus of the corresponding cylinders in Table 2 by an average of 1.83×10^6 psi at 28 days and 0.99×10^6 psi at one year. The latter figure is in excellent agreement with the Mather results in spite of the fact that the sizes of both the beams and the cylinders used in the two laboratories were different. An estimate of the shape effect may be had from the fact that the dynamic modulus of the beams in Table 1 averaged 8 percent higher than that of the cylinders at 28 days and 9 percent higher at one year. Both of the above observations are consistent with the data shown in Fig. 1.

The answers to the specific questions raised by Mr. Mather appear to be that the differences he noted are dependent more on the choice of static modulus than on the shape effect, and that the relation changes with age.

Discussion of a paper by Gerald Bowen and R. W. Shaffer:

Flat Slab Solved by Model Analysis*

By ALFRED ZWEIG†

The interesting article by Messrs. Bowen and Shaffer discussing the design of flat slabs and the accompanying example and comparison and the conclusions drawn therefrom, raises one point which, in the writer's opinion, needs to be modified.

As first item of the conclusions, the authors state: "The correct total statical moment in a uniformly loaded bay, with a coefficient of 0.125, should be used in Eq. (18), Section 1002, and Eq. (19), Section 1003, of the ACI Building Code."

There can be no doubt that this conclusion is correct and the authors are to be congratulated that they have not only formulated the issue as clearly as they did, but that they have contributed an experimental proof for a fact that has been recognized theoretically for over 40 years. J. R. Nichols of Boston published in the 1914 ASCE *Transactions* a paper in which he proved by simple statics that the coefficient in question cannot be anything else but 0.125. But to the writer's knowledge, there is not a single textbook which would accept this fact. Instead, Poisson's ratio, "experimental proof," "mysterious slab actions," and other half-mystical explanations are invoked to account for the apparent discrepancy between simple statics and a coefficient which was established in the ACI Building Code from the early beginnings of flat slab practice half a century ago.

While it is, therefore, obvious that the sum of the positive and average negative moments for uniformly distributed load in any flat slab bay taken over the center of the support and in midspan must add up to $wl^2/8$, and while it is also desirable to recognize this fact by proper revision of the code, the writer cannot agree with the further conclusions of Messrs. Bowen and Shaffer when they propose to proportion the reinforcing steel of a flat slab based on the correct theoretical moments. This is what they are doing in Table 1.

In accepting their proposal, we would disregard the practical experience of over half a century, during which time the reinforcing of tens of thousands of flat slabs was proportioned for a moment smaller than the theoretical. All these structures have proved to have an ample factor of safety. Many engineers are even inclined to attribute to the flat slab a factor of safety in

*ACI Journal, Feb. 1955, *Proc.* V. 51, p. 553. Disc. 51-30 is a part of copyrighted Journal of the American Concrete Institute, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

†Structural Engineer, Albert Kahn Associated Architects and Engineers, Inc., Detroit, Mich.

excess of other reinforced concrete structures and it would, therefore, be definitely a step backward to require an increased amount of reinforcing as suggested in the article.

The conclusion to be drawn from our experience must rather be that the concrete in flat slabs, even after cracking locally around the steel, retains a considerable tensile resistance which can be as safely and successfully utilized to reduce the total tensile reinforcing as the shear resistance of concrete is generally utilized to carry a certain specified portion of the total shear. Just as we do not hesitate to rely on the shear capacity of the concrete, we should be able to recognize the correct theoretical moments for a flat slab and still arrive at a reduced tensile reinforcing by properly attributing to the tensile resistance of the concrete a certain proportion of the total moment.

This should be done in a more direct way than it is at the present time where certain design moments are being prescribed by the code which are in obvious contradiction to the basic elements of statics, with a subsequent attempt to offset in a round-about way part of the inconsistency by including requirement in Section 1003(e) which reads: "Three-fourths of the width of the strip shall be taken as the width of the section in computing compression due to bending....." This requirement leads to a moment coefficient of $0.09/0.75 = 0.120$ for the moment to be used for the compression strength which is almost, if not entirely, theoretically correct.

If, then, the code in the final result is satisfactory by providing a sufficient reduction in the tensile reinforcing with a proved ample factor of safety, and by including a provision to account for the necessary correction in the moments to be used for checking the required compressive strength of the slab, why ask for a change of the code? The answer to this question lies in the following considerations.

1. While the ACI Building Code is satisfactory, at least in the final results, as far as the design of flat slab by moment coefficients is concerned, this is not the case when a flat slab is to be analyzed by the provision of Section 1002, which describes the procedure to be used in designing a flat slab as continuous frame. In this case, (a) the coefficient 0.09 is changed to 0.100 without subsequent modification for computing the compressive stress as prescribed for the design of flat slabs by moment coefficients, and (b) the peculiar wording of Section 1002 leads generally to positive design moments of flat slabs which are theoretically correct, but require much more (up to 28 percent) reinforcing steel than a flat slab designed with moment coefficients. It penalizes, therefore, the designer who tries to make use of this provision of the code since it does not utilize the well established qualities of concrete to resist a portion of the total moment in tension as described above.

2. The code, as written, penalizes, for the same reason, any design based on a model analysis as described for the Presan method by Messrs. Bowen and Shaffer.

For this reason, it is suggested that the ACI Building Code be revised so that:

A. The correct total statical moment in a uniformly loaded bay with a coefficient of 0.125 should be used in Eq. (18), Section 1002, and Eq. (19), Section 1003.

B. It should be assumed that part of the tensile force of the thus correctly established moments is resisted by the concrete and only the remaining part is to be resisted by the steel reinforcing:

(1) Until further investigation and tests are made, it is safe to assume that the concrete will resist 28 percent of the total moments in tension, whereas the steel shall resist the remaining 72 percent, provided the design is based on the long established moment coefficients and provided further the design is limited to the restriction of Section 1003 (a) which:

a. Limits the ratio of length to width of panel to a maximum of 1.33.
b. Requires continuity for at least three bays in each direction.
c. Provides for a maximum difference of span lengths in successive bays of not more than 20 percent to which should be added a fourth requirement which limits the ratio between live load and dead load to a maximum of say 3.

(2) For cases which are not subject to the above restrictions and which are to be designed either as continuous frames or by a model analysis, it should be assumed that the concrete will carry 20 percent of the total moment in tension and the reinforcement shall be designed to resist the remaining 80 percent.

The above differentiation between (1) and (2) seems to be justified since the analysis, limited to the restrictions of Section 1003, warrants a somewhat smaller factor of safety since it is based on the experience with an overwhelming number of flat slab structures, whereas the frame or model analysis is of much more recent date and may, therefore, require a somewhat more stringent approach.

It should also be noted that the above two suggestions try, on the one hand, to reconcile the code with the theoretical analysis of a flat slab, and on the other, to utilize the experience of over 50 years by properly reducing the amount of the necessary reinforcing steel.

C. Further studies should be made to evaluate the tensile resistance of various types of concrete and it is to be assumed that the value attributed to the tensile resistance of concrete in conjunction with the reinforcement will also be dependent upon the type of concretes used just as the shear resistance is a function of the ultimate concrete strength.

D. The beneficial effect of the tensile resistance of concrete experienced in many thousands of flat slab structures should be applied in a similar way to two-way and one-way slabs. Tests to this end should be conducted. There is no reason to assume a basically different behavior of concrete in this respect in a flat slab than in a two-way slab or in a one-way slab.

E. If the tensile resistance of a flat slab is the reason for the reduction of the moment coefficients in the present ACI Building Code, and it seems

to be the only rational explanation for the successful experience with this type of construction, Section 1001 (b) should be questioned when it states: "Recesses, or pockets in flat slab ceilings, located between reinforcing bars, and forming cellular or two-way ribbed ceilings, whether left open or filled with permanent fillers, shall not prevent a slab from being considered a flat slab."

This type of construction (ribbed flat slab) is gaining in popularity, but in the light of the above approach, it is doubtful whether the concrete in such a floor—at least as far as the positive moments resisted by the narrow ribs are concerned—should be credited with as much tensile resistance as a slab with uniform thickness. Experiments to this end should be conducted before definite conclusions can be reached.

Disc. 51-34
(covering also 51-15, 51-21, and 51-28)

Discussion of a series of papers by K. G. Moody, I. M. Viest, R. C. Elstner, and E. Hognestad:

Shear Strength of Reinforced Concrete Beams*

By E. J. RUBLE, J. TAUB, and AUTHORS

By E. J. RUBLE†

The authors are to be congratulated on the wealth of data accumulated and presented in the first three papers of the series. Their analysis of the data and analytical studies of the action of reinforced beams failing in shear in Part 4 will be of tremendous help to designing engineers in evaluating the importance of various factors.

As a research engineer working closely with designing engineers and specification writing bodies, I was interested in a comparison of the laboratory results obtained by several investigators with various equations which have been proposed to predict the shear strength of reinforced concrete beams. My discussion will consist principally of showing these comparisons and the influence of such factors as the shear span, effective depth, longitudinal reinforcement, web reinforcement, and concrete strength. The first four figures show the comparisons for the simple span beams without web reinforcement while the last four are for simple span beams with web reinforcement.

In Fig. A, as well as the following diagrams, the maximum average shear from the test results divided by bjd is shown by the abscissa while the unit shear stress predicted by the proposed equation at ultimate load is shown by the ordinate. In this particular diagram, the actual shear stress at ultimate load is compared with the equation $v_u = 0.10\ f_c' + 5000\ p$, where f_c' is the ultimate concrete strength and p is the percentage of longitudinal reinforcement. It should be noted that this equation predicts the ultimate shearing stress based on only two factors, f_c' and p. For many of the beams tested this equation predicts ultimate shear stresses considerably higher than that obtained by actual tests. This is noticeable by the large number of points above the 45-deg line. If the equation had predicted the actual shearing stress, the points would have fallen along the 45 deg-line. For example, a large number of the beams failed at a shearing stress between 200 and 250 psi but the predicted values by this equation were between 600 and 700 psi.

*ACI JOURNAL, *Proc.* V. 51: "Part 1—Tests of Simple Beams," Dec. 1954, p. 317; "Part 2—Tests of Restrained Beams Without Web Reinforcement," Jan. 1955, p. 417; "Part 3—Tests of Restrained Beams With Web Reinforcement," Feb. 1955, p. 525; "Part 4— Analytical Studies," Mar. 1955, p. 697. Disc. 51-34 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

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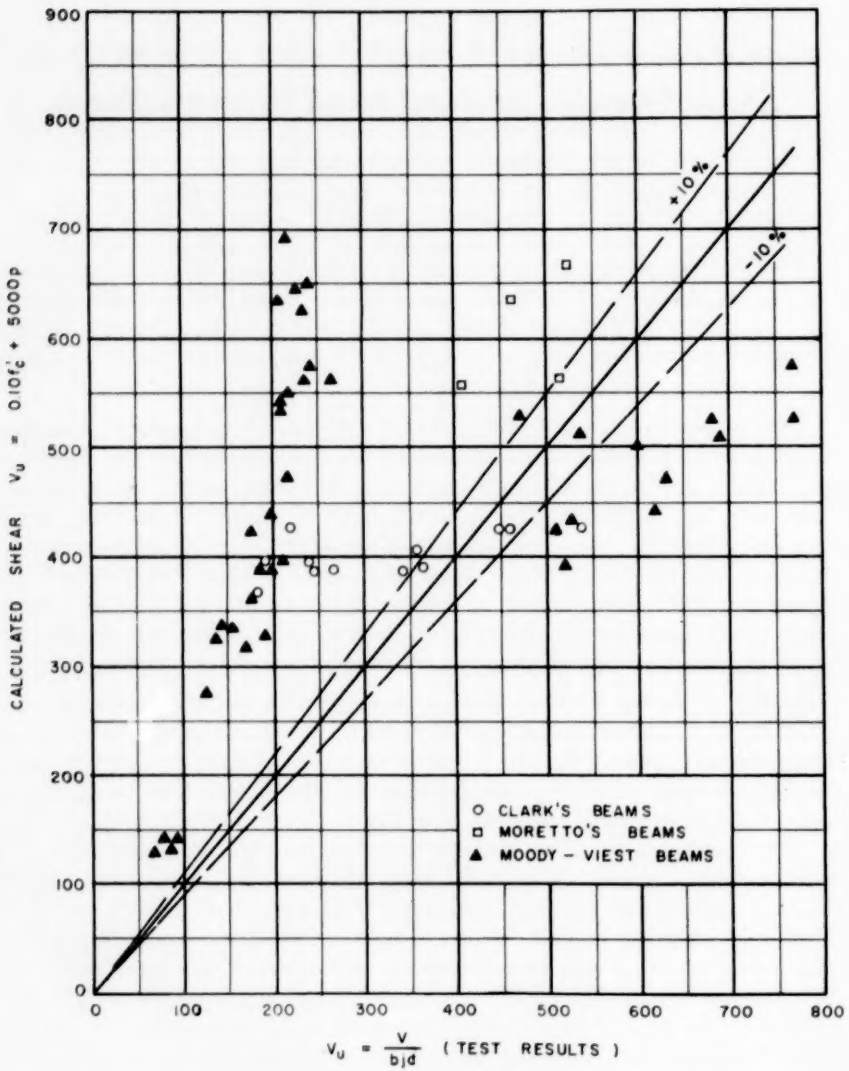

Fig. A—Simple beams without web reinforcement

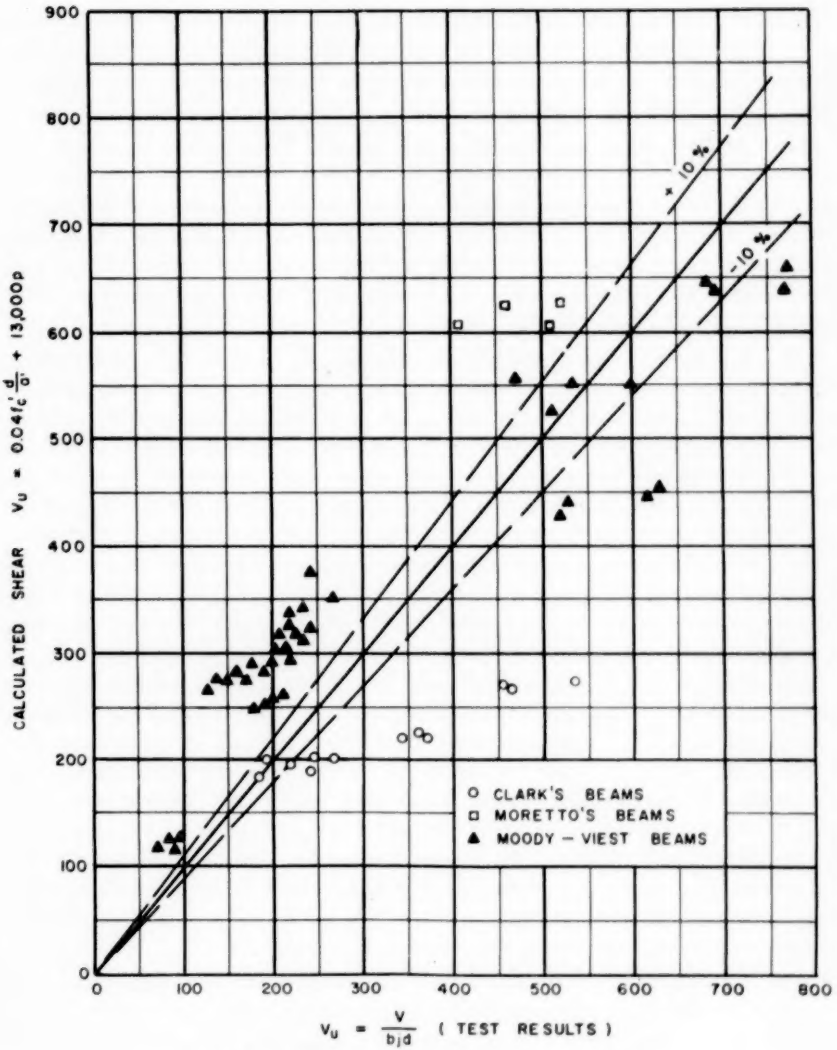

Fig. B—Simple beams without web reinforcement

Fig. B is similar to Fig. A. Here, however, the ordinates are unit shear stresses predicted by the equation $v_u = 0.04\ f_c'\ (d/a') + 13{,}000\ p$. This equation is in form similar to the equation for Fig. A with the introduction of the factor d/a'.

This factor is the ratio of the effective depth of beam to the effective shear span. It is seen that the points are closer to the 45-deg line, indicating that the shear span has a pronounced influence on the strength of the beams.

Fig. C, like Fig. B, has ordinates predicted by the equation $v_u = 0.12\ f_c'\ (d/a) + 7000\ p$ which is in the same general form as the equation just described. This equation also utilizes the effect of the ratio of effective depth to the shear span, but increases the effect of the concrete strength and decreases the longitudinal reinforcement effect. Predicted values are close to test results but there is still considerable scatter, with some of the predicted values 50 percent greater than the actual values while others are only about 70 percent of the actual values.

Fig. D shows how the Moody-Viest equation, in predicting ultimate shear stress, compares with the test results. Plotted points are noticeably closer to the 45-deg line for the complete range of stress. This equation utilizes all of the factors known to influence the ultimate strength of beams without web reinforcement failing in shear, previously mentioned by the authors, namely, concrete strength, shear span, percentage and stress of the longitudinal reinforcement, dimensions of the cross section, and properties of the stress block. Unlike the three previous equations, this one considers the bending moment M_s at the critical section of a beam at shear failure, and with the aid of this ultimate moment equation the shear strength of the beam may be predicted. In addition to p, f_c', b, and d, the ultimate moment is dependent upon f_s, k_1, k_2, and k_3, where f_s is the stress in the longitudinal reinforcement at shear failure; k_1, k_2, and k_3 are parameters which describe the shape of the concrete stress block in the compression zone at failure. It can be seen that this equation, though more complex than the other three just described, does predict shear strengths closer than those equations in the simpler form. It must be noted, also, that more predicted shear strengths by this equation are *less* than those obtained by the test results. This is apparent by the predominance of plots below the 45-deg line. This means, in effect, that beams designed for ultimate strength by this equation will have a slightly higher actual load factor than was used in the design.

These four diagrams have shown how predicted shear strengths computed by various equations compare with actual test results for simple beams *without* web reinforcement. The next four diagrams will show the same comparison for beams *with* vertical web reinforcement.

In Fig. E the ordinates are plotted for the predicted shears using the equation $V_u = 0.10\ f_c' + 5000\ p + k'rf_{vy}$. The term $k'rf_{vy}$ has been added to reflect the effect of the web reinforcement. Without this term the equation is as shown on Fig. A. The factor k' varies with the slope of the web reinforcement,

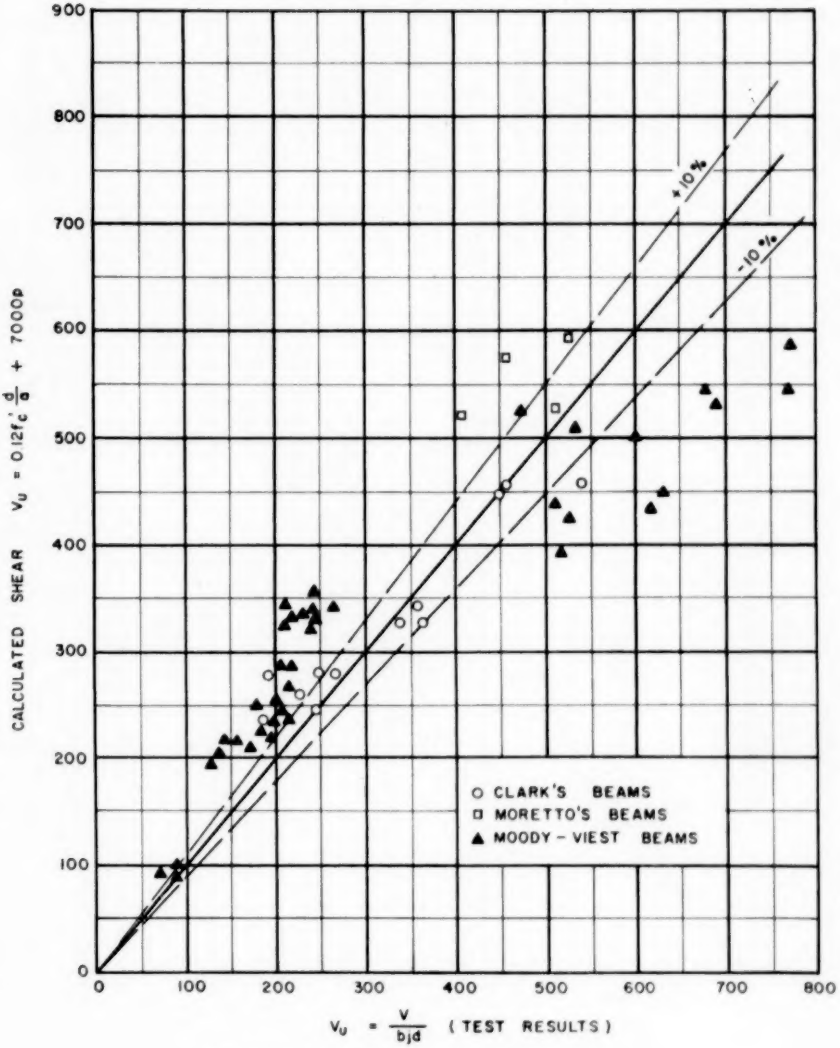

Fig. C—Simple beams without web reinforcement

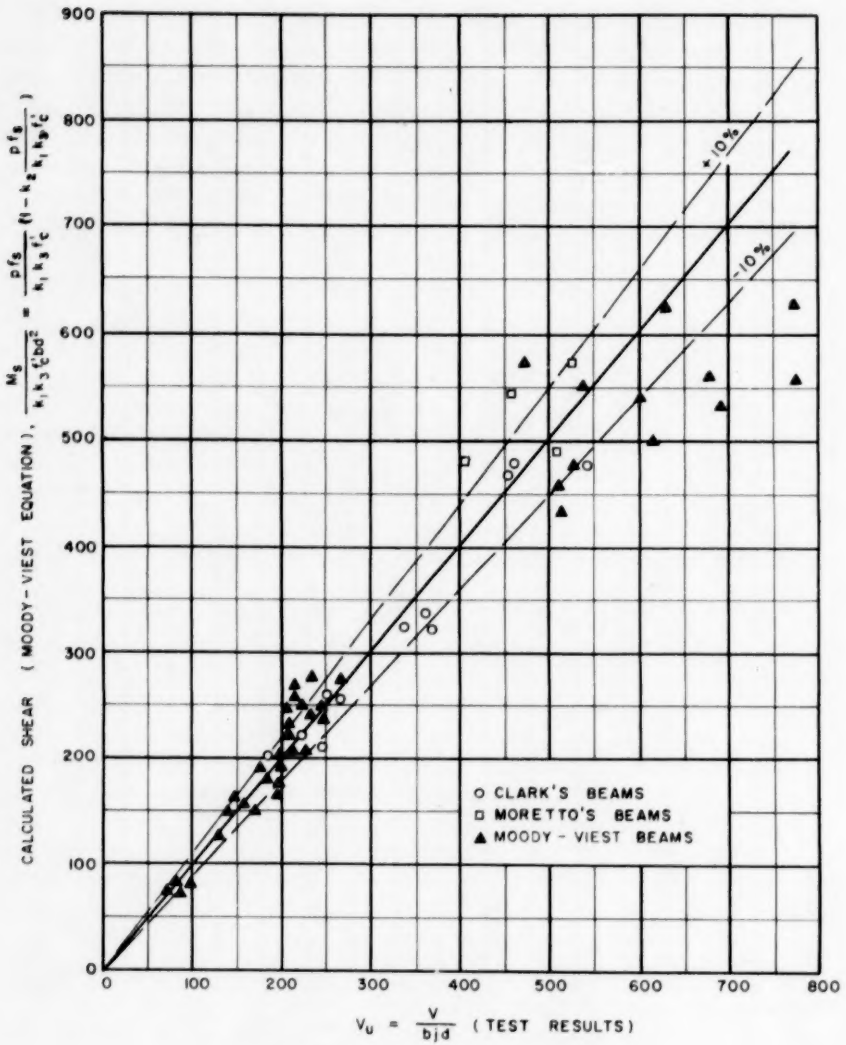

Fig. D—Simple beams without web reinforcement

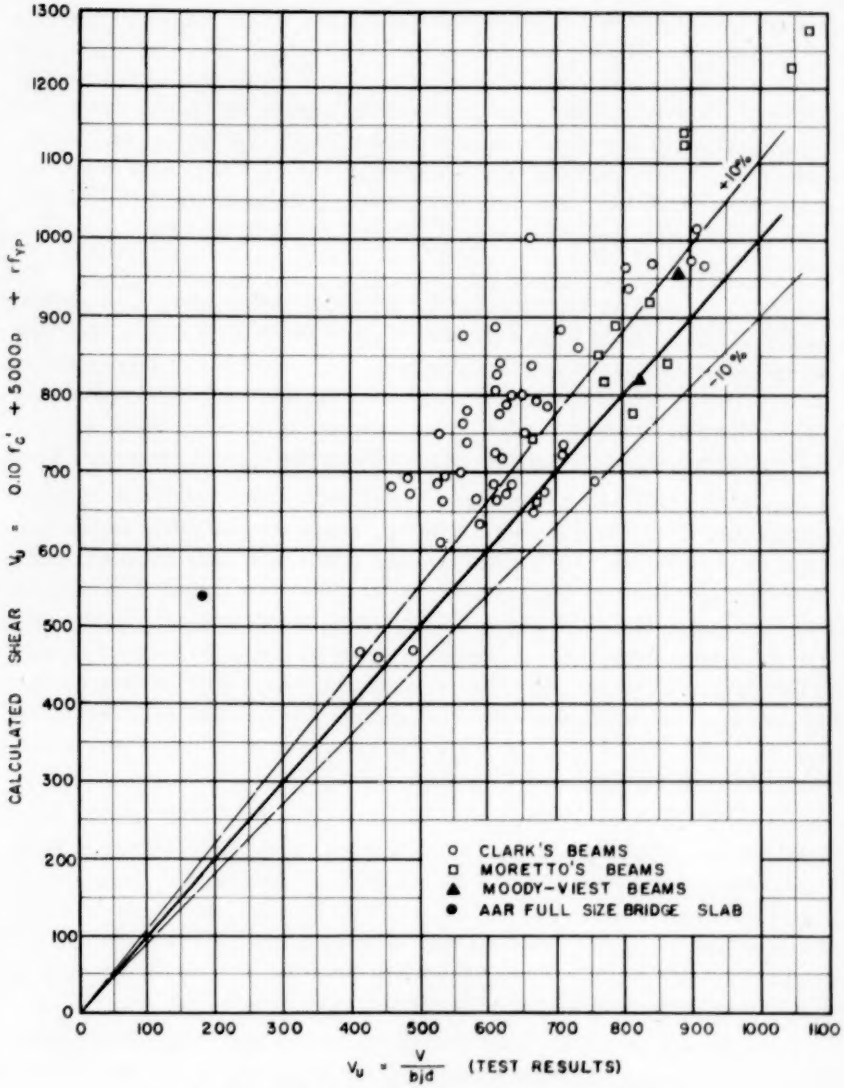

Fig. E—Simple beams with vertical web reinforcement

To agree with notation of paper f_{vp} should read f_{vy}

which for vertical stirrups equals 1.0; r is the ratio of the web reinforcement and f_{vy} is the yield point stress of the web reinforcement. It is noted by the large number of plotted points above the 45-deg line that some predicted values are 50 percent greater than the test results. Also shown on the diagram by the solid circle is the comparison of predicted shear strengths and test results of the full size railroad bridge slab tested by the Assn. of American Railroads. This slab was designed according to current specifications and at failure exhibited a typical diagonal tension crack that progressed to the section of maximum moment and resulted in the destruction of the compression zone. However, this equation predicts a shearing strength of about 540 psi but the slab actually failed at 189 psi, or about $\frac{1}{3}$ of the predicted value.

In Fig. F the ordinates are plotted for predicted shears using the equation as on Fig. B but with the term $k'rf_{vy}$ added. The terms k', r, and f_{vy} have the same significance as in the previous diagram. Here again are a large number of plotted points above the 45-deg line indicating that the predicted values are higher than actual tests results. This equation predicts the shear strength of the AAR slab about 35 percent greater than was found from actual test.

In Fig. G the ordinates are plotted for predicted shears using the equation shown on Fig. C but with the term $2500\sqrt{r}$ added for the effect of the web reinforcement; r in this case is also the ratio of the web reinforcement. The wide scatter of values so apparent on the two previous diagrams is not apparent here and most values are within plus or minus 10 percent. In the case of the AAR slab, however, the predicted value is about 70 percent greater than test results. The fact that these equations do not predict closely for the AAR slab may be due to size effect. This full size railroad slab was 24 in. deep, 78 in. wide, and 19 ft long, while the laboratory test beams were much smaller, 10 to 24 in. deep and 6 to 8 in. wide.

Fig. H is the Moody-Viest equation but the factor $T_v\bar{x}/k_1k_2f_c'\,bd^2$ has been added to reflect the contribution of the vertical web reinforcement. T_v is the total tension force in the web reinforcement crossing a diagonal crack and $\bar{x}$ is the longitudinal distance defining the location of T_v. It is noted that this equation predicts values close to $\pm$ 10 percent of the test results. This equation also predicts more values less than the test results which is a desirable feature in determining ultimate strength.

The comparison of values just shown illustrates that the Moody-Viest equation predicts ultimate shear strength of reinforced concrete beams within reasonable tolerance. Apparently the approach to the problem through moment resistance leads to a more generally applicable solution. The resulting equation is somewhat complex in its present form, but it is possible that some simplifications may be affected to permit the designer to use it more readily.

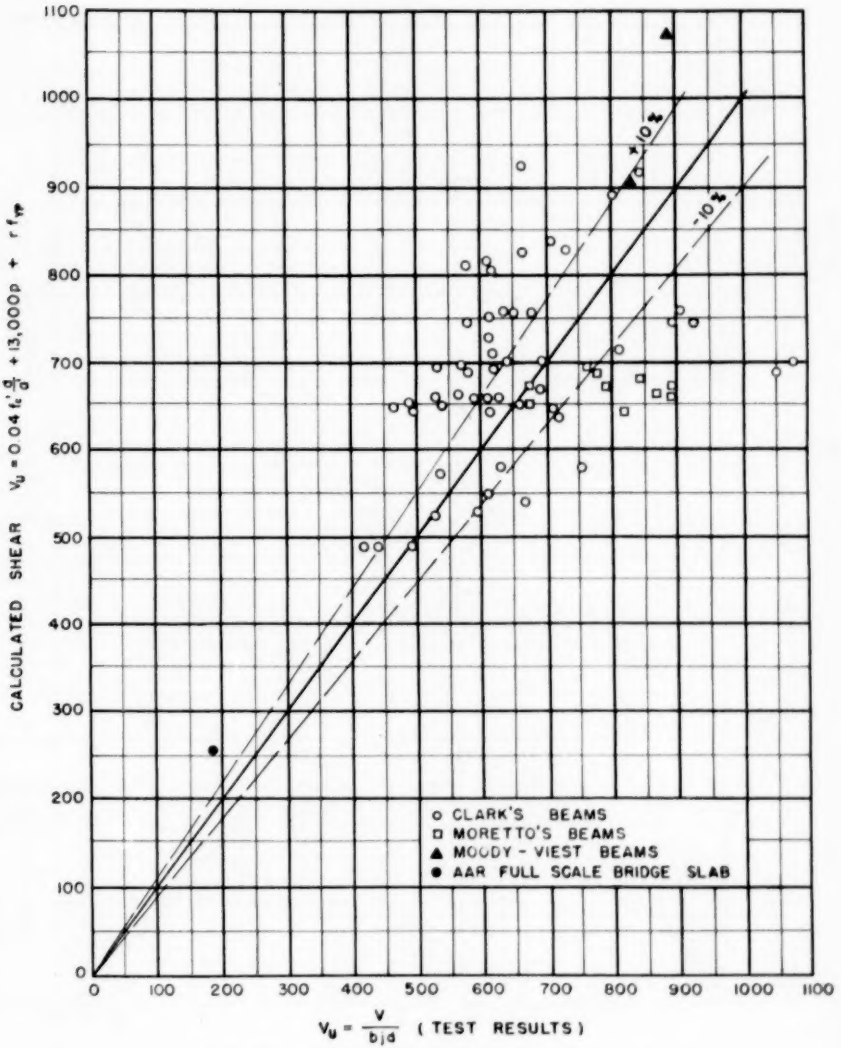

Fig. F—Simple beams with vertical web reinforcement

To agree with notation of paper f_{vy} should read f_{sy}

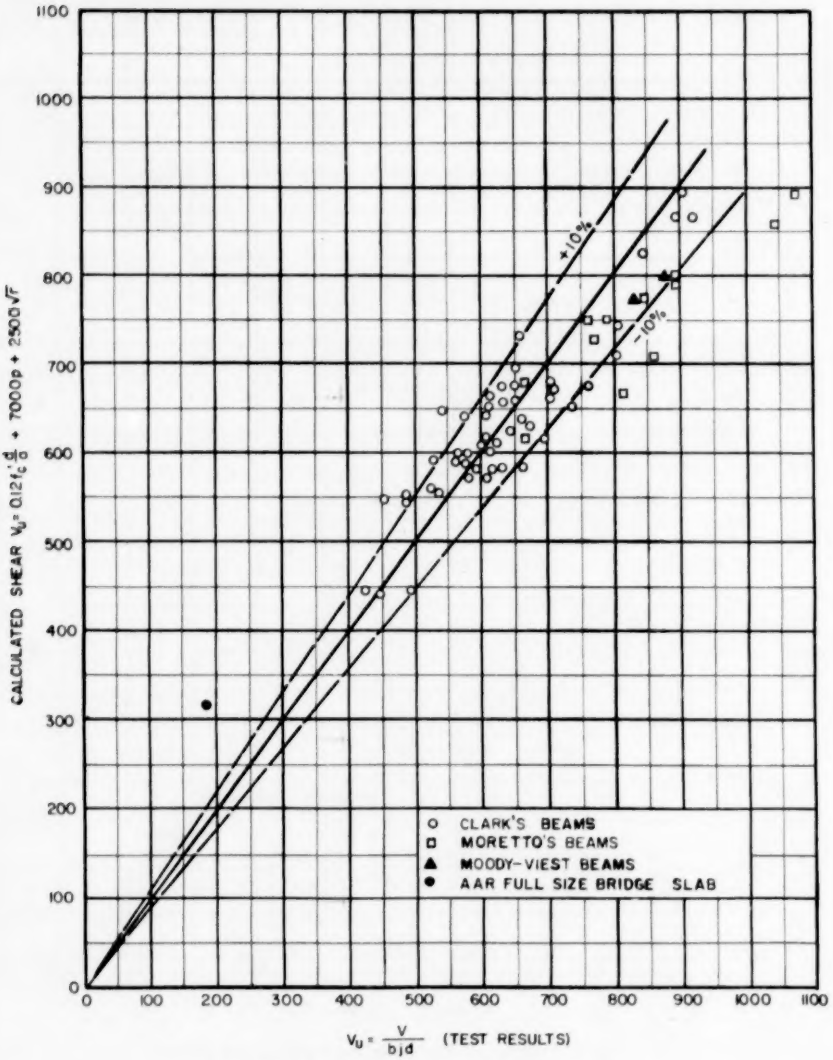

Fig. G—Simple beams with vertical web reinforcement

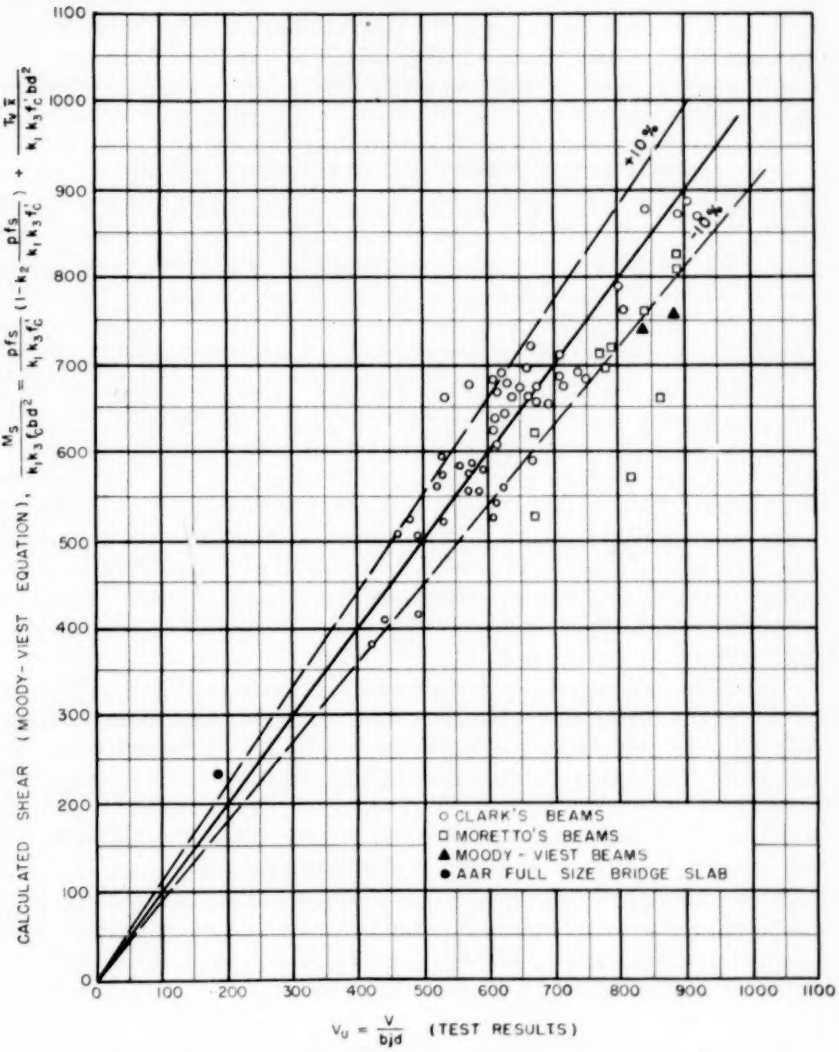

Fig. H—Simple beams with vertical web reinforcement

By J. TAUB*

Messrs. Moody, Viest, Elstner, and Hognestad have ably reported on their extensive investigations of shear strength of reinforced concrete beams. The report was especially interesting to the writer as he is working on a book on shear reinforcement in reinforced concrete beams, which is based mainly on German tests and which will discuss a new theory of the action of this reinforcement. Some of the following remarks are based on the theses of this book.

SIMPLE BEAMS WITHOUT WEB REINFORCEMENT (PART 1)

The deformation of a simply supported reinforced concrete beam after the formation of a diagonal tension crack, according to Mörsch[1], is shown in Fig. 1. To satisfy the conditions of equilibrium the reaction at the support $P/2$ and the internal forces C and T must meet at a point; forces C and T are, therefore, inclined to the axis of the beam. The shear force is taken partly by the concrete and partly by the tension reinforcement.

At this stage, the tension steel at the point crossing the diagonal tension crack is pressed down by the vertical component of the force T, and this causes a horizontal crack and splitting of the beam along the tension reinforcement from the diagonal crack up to the support. The dowel action of the deformed bars precipitates the developing of this crack but it does not cause its occurrence, as such horizontal cracks along the tension steel were also observed in German tests with beams that were reinforced with plain bars.[2] After formation of this horizontal crack, bond between tension reinforcement and concrete was completely destroyed, and this led to failure of the beam.

Even when the diagonal tension crack did not reach the bottom surface of the beam but extended along the tension steel, the same action took place. Although, for some beams, ultimate load was equal to cracking load there was some continuation in development of the cracks. The diagonal crack was formed first; as the bottom end of the crack was wider than the top end, it caused the pressing down of the tension steel at the crossing with it and the occurrence of further phenomena as described.

The writer agrees with the concluding remarks of the authors that magnitude of the failure loads depended primarily on dimensions of the cross section,

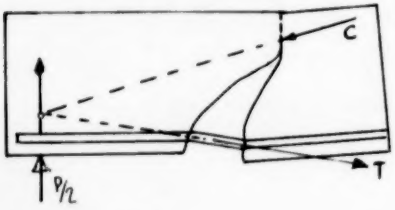

Fig. 1—Deformation of simply supported beam after formation of diagonal tension crack according to Mörsch

*Tel-Aviv, Israel.

amount of longitudinal reinforcement, and strength of concrete. He does not agree with the assertion that ultimate load may be predicted on the basis of the ultimate moment independently from the magnitude of shear (pp. 331-332). This assertion was based on the equations:

$$\frac{v_u}{f_c'} = A\,\frac{d}{a} \qquad\qquad v_u = 8V_u/7bd$$

The first equation, was based on the assumption that there was a direct proportionality between the ratios of the effective beam depth to the shear span (d/a) for the Series B, A, and III and the respective ratios of v_u/f_c' (p. 330). In the opinion of the writer, it is not possible to draw direct conclusions in this respect from the test results, as not only the concrete strength but also the amount of the tension steel were different for the three series. But if we assume that the greater concrete strength compensates for the smaller percentage of the reinforcement we could compare, for instance, the test results of beams B-9, A4, and III-27a, for which the differences in percentage of reinforcement are relatively small. The ratios d/a for these beams were 1:1.14:2.28 whereas the respective ratios v_u/f_c' were 1:1.50:5.50. There was, therefore, a great difference between these ratios.

The second of the above equations is the normal formula for nominal shear stress. Since no stresses can exist along the cracks, this equation is not valid for the ultimate load.

RESTRAINED BEAMS WITHOUT WEB REINFORCEMENT (PART 2)

The failure of these beams resulted from destruction of the compression zone after the formation of one or two major diagonal tension cracks.

An instructive picture of the behavior of these beams is given by Fig. 12 of Part 2 (p. 432) in which measurements of strains in the longitudinal reinforcement, before and after formation of diagonal tension cracks, for beam I-2c are registered. In this beam two major diagonal tension cracks formed. Measurements taken up to the formation of the second crack give the behavior of a beam in which only one crack formed, the further measurements demonstrate the behavior of a beam with two diagonal cracks.

Beams with one major diagonal tension crack

The initial diagonal tension crack formed in beam I-2c at a load of about 50 kips. Strains measured in the bottom reinforcement demonstrated that at section *a-a* this reinforcement was strained in compression up to a load of 55 kips, beyond this load up to failure it was strained in tension. Strains measured in the top reinforcement at section *b-b* showed that the diagonal tension crack was much wider at its top end than at the level of the bottom reinforcement.

Fig. J shows the deformation of a beam with one fully developed diagonal crack and the action of forces on the portion located to the left of the crack. As seen from this figure the equation $T_1 jd = M_a$, given on p. 430 of Part 2, seems to be inaccurate.

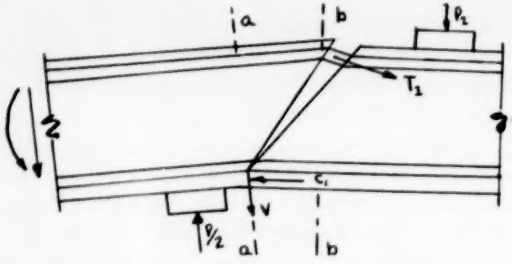

Fig. J—Deformation of restrained beam with one fully developed diagonal crack

Beams with two major diagonal tension cracks

The second diagonal tension crack in beam I-2c formed at a load of 80 kips. Failure occurred at a load of 94 kips. Strains measured in the top reinforcement demonstrated that at a load of 65 kips and afterwards up to failure this reinforcement at section *b-b* was strained in tension. The second diagonal tension crack was wider at its bottom end than at the level of the top reinforcement. Fig. K shows the deformation of a beam with two major diagonal tension cracks and the action of forces on the portion located between the two diagonal cracks. From this figure it is seen that the equation $T_2 jd' = M_b$ seems to be incorrect for these beams.

As to the authors' concluding remarks, the writer agrees with the assertion that magnitude of the ultimate loads of restrained beams without web reinforcement depended primarily on dimensions of the cross section, amount of longitudinal reinforcement, and concrete strength. But for the same reasons as given when discussing simple beams he cannot accept the assertion that magnitude of the ultimate load depended directly on the ratio M/Vd.

The writer thinks, too, that the tests that were made do not suffice to draw the conclusion that the ratio of the ultimate load to the cracking load decreases with increasing ratio of shear span to effective depth. This conclusion was based on the observation that for the beams of Series IV the difference between initial cracking load and ultimate load was smaller than for beams of Series I and II. In considering this question it is to be taken into account that at equal loads the positive and negative moments are smaller for the beams of Series I and II than for the beams of Series IV; the shear forces, however, are greater. This is believed to cause the earlier

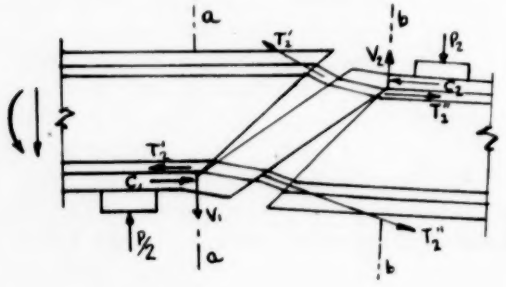

Fig. K—Deformation of restrained beam with two major diagonal tension cracks

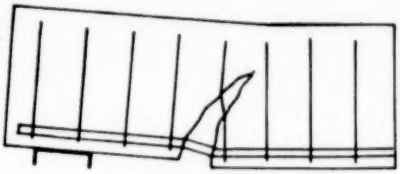

Fig. L—Deformation of beam with vertical stirrups after formation of diagonal crack

formation of the initial diagonal cracks in the beams of Series I and II, and the attainment of higher ultimate loads of these beams than of the beams of Series IV.

SIMPLY SUPPORTED AND RESTRAINED BEAMS WITH WEB REINFORCEMENT (PARTS 1 AND 3)

Vertical stirrups

The vertical stirrups fulfill two different tasks in a reinforced concrete beam. The first task is to act as shear reinforcement when a diagonal tension crack occurs. Deformation of a reinforced concrete beam with vertical stirrups after formation of the diagonal crack is represented in Fig. L. The correctness of this representation follows from Fig. 2 (left) in Part 3 (p. 531) which shows a typical beam of Series I after failure.

Fig. M shows, to enlarged scale, the action of the stirrup forces at the crossing with the diagonal tension crack. An inclined force T_s acts on the upper part of the stirrup. This force can be resolved into two components; the vertical component T_s^v acts as tension force on the upper part of the stirrup, the horizontal component T_s^h produces shear stresses in the stirrup at the crossing with the diagonal crack. The action of forces on the bottom part of the stirrup is the same but in the opposite direction. Forces T_s^v and T_s^h represent two balanced couples.

The magnitude of the stirrup forces can be calculated as follows. Fig. N shows the left portion of the beam and the forces acting on it immediately after formation of a diagonal tension crack, before it opened widely.

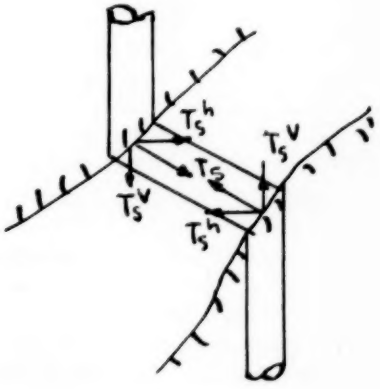

Fig. M—Stirrup forces at crossing with diagonal tension crack

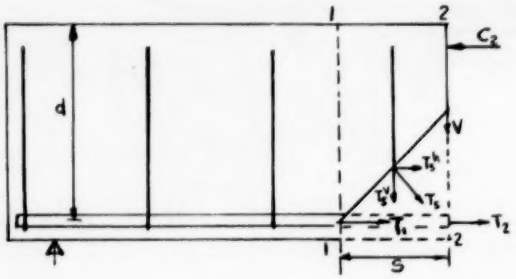

Fig. N—Forces acting on beam with vertical stirrups immediately after formation of diagonal tension crack

The vertical component of the stirrup force T_s^v is

$$T_s^v = \frac{V_s}{jd}$$

V_s equals the increase of the bending moment between sections 2-2 and 1-1; therefore

$$T_s^v = \frac{M_2 - M_1}{jd}$$

Since

$$\frac{M_2}{jd} = T_2$$

and

$$\frac{M_1}{jd} = T_1$$

we obtain

$$T_s^v = T_2 - T_1 \quad \ldots\ldots (1)$$

The magnitude of the horizontal component of the stirrup force is determined from the condition of equilibrium:

$$T_1 + T_s^h = C_2$$

Since

$$C_2 = T_2$$
$$T_1 + T_s^h = T_2$$

or

$$T_s^h = T_2 - T_1 \quad \ldots\ldots (2)$$

and

$$T_1 = T_2 - T_s^h \quad \ldots\ldots (3)$$

The inclined stirrup force T_s is, therefore

$$T_s = (T_s - T_1)\sqrt{2} = 1.41\,(T_2 - T_1) = 1.41\,\frac{V_s}{jd} \quad \ldots\ldots (4)$$

At some load the stresses in the stirrups reach the yield point, and then their strains approach the plastic range. With further increases of load the diagonal crack opens wider, elongation of the stirrups increases, but the force taken by the stirrups remains unchanged; the role of the stirrups as shear reinforcement has come, therefore, to an end. Hence it follows that at this stage, a

reinforced concrete beam, the shear reinforcement of which consists of stirrups, behaves like a beam without web reinforcement, and from now on a redistribution of internal forces takes place. Since with further increases of load magnitude of the force T_s^h remains constant, it follows from Eq. (3) that all increases in tension force T_2 at section 2-2 produce equal increases in tension force T_1 at section 1-1. The force T_1 now becomes much greater than the value obtained from the calculation made on the basis of elastic theory.

The correctness of the above description follows from Fig. 3 in Part 1 (p. 325). Measurements of stirrup strains are shown in this figure for beam III-30 with 0.52 percent web reinforcement and beam III-31 with 0.95 percent web reinforcement. In beam III-30 the yield point of the stirrups was reached at a load of 135 kips, whereas its failure load was 215 kips. In beam III-31 the yield point of the stirrups was reached at a load of 170 kips and its failure load was 228 kips.

Redistribution of internal forces begins in beams without web reinforcement after formation of the diagonal tension crack, and in beams with web reinforcement after yielding of the web reinforcement. In either case redistribution of internal forces ends at failure.

After the redistribution of internal forces, the magnitude of the failure load depends on the resistance of the tension and compression zones at both ends of the diagonal tension crack.

Thus failure of a simply-supported rectangular beam, with its weak compression zone relative to the compression zone of a T-beam, results from the crushing of concrete at the top end of the diagonal crack. All the rectangular beams described by the authors failed in this way. Failure of a simply-supported T-beam with its strong compression zone results from the destruction of the beam and by tension steel hooks following bond failure. All the T-beams tested by Bach and Graf[2] failed in this way.

The stirrups fulfill still another function in a reinforced concrete beam quite independent from that described above, which does not manifest itself at the diagonal tension crack but outside of it, and which is of great importance for the strength of the beam and the magnitude of its failure load.

As was stated before, after formation of a diagonal tension crack in a beam without web reinforcement the tension steel is pressed down at the bottom end of the crack, causing a horizontal crack and splitting of the concrete along this steel. The bond between concrete and steel is, therefore, destroyed, and that leads to immediate failure of the beam (see Fig. 4 and 5 in Part 1, pp. 325 and 326). If, however, the beam has vertical stirrups, its behavior is quite different. The stirrups prevent the pressing down of the tension steel and the splitting of the concrete between the diagonal crack and the beam support. The monolithic connection between concrete and steel is, therefore, maintained, and the beam is able to sustain further loading (see Fig. 4 in Part 1). This action of the stirrups is quite independent from their dimensions; stirrups of small diameter are able to fulfill this task as well as

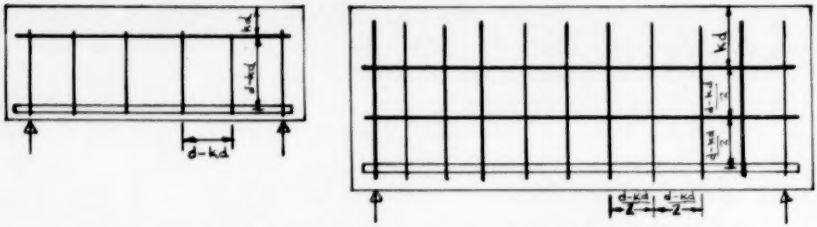

Fig. O—Examples of beams with stirrups and horizontal web reinforcement

those of greater diameter. That was one of the reasons why the difference between maximum loads of beams III-31 and III-30 was so small. The amount of stirrups in beam III-31 was 83 percent greater than in beam III-30, but the failure load of beam III-31 was only 6 percent greater than that of beam III-30.

Let us now discuss ways of improving the action of vertical stirrups in a reinforced concrete beam. According to the two roles of these stirrups, there are two ways of doing it. The first task of the stirrups is to act as shear reinforcement when the diagonal tension crack is formed. In this respect, vertical stirrups are not able to take all the diagonal tension force. As they do not lie in the beam in the direction of the action of this force they are not able to carry the horizontal component of this force. By adding additional horizontal reinforcement to the vertical stirrups it may be possible to increase the shear strength of the beams.*

Fig. O shows two examples of such beams with one and two additional horizontal rows of steel, respectively. If the shear force is large the horizontal reinforcement could be arranged in still more rows. The top steel is placed at the level of the neutral axis; additional rows are arranged at equal distances between neutral axis and bottom tension reinforcement. Spacing of the stirrups is equal to the spacing of the horizontal steel.

The action of the stirrups and the horizontal steel is discussed here on the basis of a beam with one additional horizontal steel placed at the level of the neutral axis.

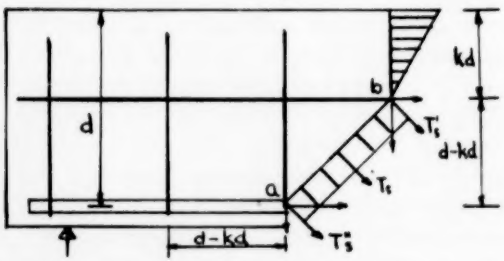

Fig. P—Beam with stirrups and one additional row of horizontal web reinforcement

*The arranging of additional horizontal reinforcement was also discussed by Rausch;[3] his approach to this question is, however, different than that of the writer.

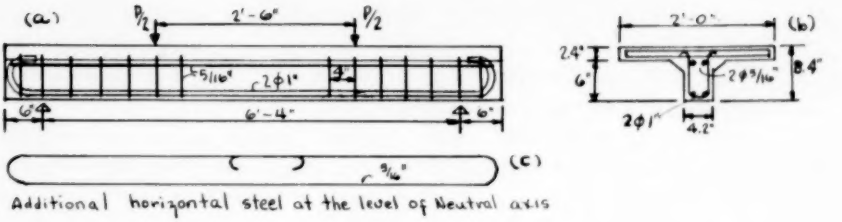

Fig. Q—Test beam

Fig. P shows the left portion of such a beam. The diagonal tension force T_s can be resolved into two components, T_s' and T_s''. The component T_s' is taken by stirrup b and the additional horizontal steel, the component T_s'' by stirrup a and the longitudinal reinforcement.

After formation of a diagonal crack only the vertical components of the diagonal tension force act on the stirrups. Their stresses are, therefore, smaller than in the beams which have no additional horizontal steel. This steel carries a part of the horizontal component of the diagonal tension force. The stress in the longitudinal reinforcement is accordingly smaller. Hence it follows that the strength of such a beam ought to be greater than that of a corresponding beam without additional horizontal reinforcement.

To get experimental confirmation of the above thesis, two tests were made by the writer. The tests were carried out in 1947 at the Building Materials Testing Laboratory, Technion—Israel Institute of Technology, Haifa.

Fig. Q shows the test beam with additional horizontal steel at the level of the neutral axis. Fig. Q(c) shows the arrangement of this steel. Except for the absence of additional horizontal steel the second test beam was identical with that represented in Fig. Q. Two specimens of each beam were made. The failure load of this latter beam was 31 kips, that of the beam with additional steel 39 kips (average figures from two companion specimens). The beams without additional horizontal steel at the level of the neutral axis failed in shear. At failure the beam end at the support was destroyed by the action of the hooks of the tension steel. Failure of the beams with additional horizontal steel was due to the simultaneous effect of tension in the main steel and diagonal tension. This test therefore confirmed the correctness of the writer's thesis.

If the additional horizontal steel is arranged in more rows, it must be taken into account that they have to also carry tension stresses produced by flexure; their area is, therefore, to be calculated accordingly.

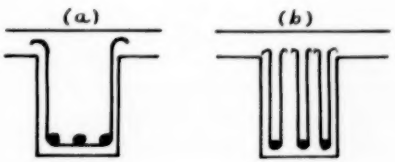

Fig. R—Types of vertical stirrups

The second task of vertical stirrups is to prevent the pressing down of the longitudinal reinforcement at the bottom end of the diagonal tension crack and the splitting of the concrete along the tension steel. Fig. R shows two types of vertical stirrups. The stirrups represented in Fig. R(b) are, of course, more efficient in this respect than those shown in Fig. R(a). A confirmation of this assertion can be found in German tests,[2] but it would be expedient to make further tests.

Inclined stirrups

Inclined stirrups at 45 deg proved to be more effective as they lie in the beam in the direction of the action of diagonal tension. It is known, that by means of 45-deg bent-up bars or stirrups, if their area and spacing are chosen correctly, it is possible to attain full safety against the action of diagonal tension. In the tests described in Part 3 the inclined stirrups were suitably spaced, but they were not strong enough to carry all the forces resulting from diagonal tension.

Fig. S shows the left portion of such a beam and the forces acting on it immediately after formation of a diagonal tension crack, before it opened widely. The magnitude of force T_s acting on the inclined stirrups is

$$T_s = bsv\ \sqrt{2}$$

Since

$$v = \frac{V}{bjd}$$

$$T_s = \frac{V_s}{jd}\ \sqrt{2}$$

Since

$$V_s = M_2 - M_1$$

$$T_s = \frac{M_2 - M_1}{jd}\ \sqrt{2} = 1.41\,(T_2 - T_1) \qquad (5)$$

If we resolve the diagonal force T_s into a vertical and horizontal component we obtain

$$T_s^v = T_s^h = T_2 - T_1 \qquad (6)$$

or

$$T_1 = T_2 - T_s^h \qquad (7)$$

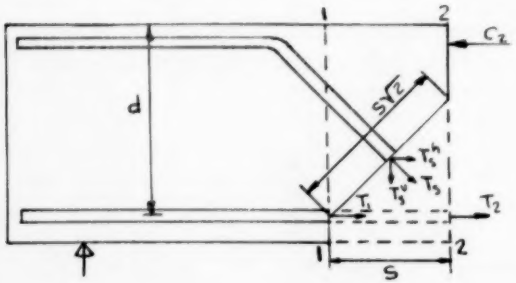

Fig. S—Forces acting on beam with inclined stirrups immediately after formation of diagonal tension crack

These are the same equations that applied to the action of vertical stirrups, and the conclusions reached then also apply here.

If the area of the inclined stirrups is not sufficient, at some load the stirrup stresses reach the yield point and then their strains approach the plastic range. A redistribution of internal forces now takes place which makes further increase of load possible. From Fig. 8 in Part 3 (p. 536) it is seen that the failure load of inclined stirrups was greater than the load at which these stirrups were yielding.

The tests of restrained beams also included two T-beams with vertical stirrups. These tests indicated that T-beams possess higher shear strength than corresponding rectangular beams. No final conclusions can be drawn, however, as only two tests do not give a full picture of the behavior of these beams. It is hoped that the excellent research work of the authors will be continued in this direction.

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2. Bach, C., and Graf, O., "Versuche mit Eisenbeton-Balken zur Ermittlung der Widerstandsfähigkeit verschiedener Bewehrung gegen Schubkräfte," *Bulletin* No. 10, Part 1, Deutscher Ausschuss für Eisenbeton, Wilhelm Ernst & Sohn, Berlin, 1911.

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AUTHORS' CLOSURE

The investigation was carried out in two parts. After the conclusion of the first part, studies of the results were aimed at establishing a general equation for the nominal shear stress at shear failure. Although it was found possible to express the ultimate nominal shearing stress by equations of the type discussed by Mr. Ruble, no single equation applicable to both simple and restrained beams could be established. Therefore, the empirical approach through the nominal shearing stress was abandoned and a more rational approach was sought through studies of the behavior observed in the tests. The need for a rational approach to the problem of the shear strength of reinforced concrete beams is emphasized by both discussors.

Mr. Ruble compares the shear strengths of simple span beams obtained in four experimental investigations with equations proposed by various authors and concludes that ". . . . the approach to the problem through moment resistance leads to a more generally applicable solution." This point is even more apparent if similar comparisons are made for restrained beams.

The phenomenological approach to the problem is followed by Dr. Taub, whose discussion of the behavior of beams with web reinforcement is especially instructive. The basic concepts of Dr. Taub's explanation of the behavior are essentially the same as those advanced by the authors, namely that the presence of an inclined crack is a prerequisite for a shear failure and that

redistribution of internal stresses takes place after formation of the inclined crack. Nevertheless, there are some important differences between Dr. Taub's and the authors' views. These may be divided into three groups: (1) the dowel action of reinforcing bars, (2) the effect of the M/Vd ratio on shear strength, and (3) shear transfer by vertical stirrups.

In Fig. I, J, and K the tensile forces in the reinforcement at the diagonal crack are shown as inclined forces. This is so because the reinforcement is able to transfer a portion of the vertical shear through dowel action. There is no doubt regarding the presence of this portion of vertical shear, but the question may be raised whether it is large enough to warrant inclusion in analytical considerations. The amount of vertical shear resisted by the longitudinal reinforcement is limited by the tensile strength of the concrete beam at the level of the steel; an amount in excess of this limiting value causes splitting of the beam along the reinforcement. As the tensile strength of concrete is low and the vertical tensile stresses are concentrated near the plane of the diagonal tension crack, usually the contribution of the longitudinal reinforcement to the transfer of shear cannot be large. It is believed, therefore, that ordinarily shear resistance of longitudinal reinforcement may be neglected in the analysis.

The comparisons between the v_u/f_c' ratios for beams of different M/Vd ratios, discussed on pp. 330 and 433 and questioned by Dr. Taub, are valid only for beams with *equal strength of concrete and equal percentages of longitudinal reinforcement.* The following table includes three restrained and three simple beams having a relatively narrow spread of f_c' and p:

Specimen	Concrete strength f_c', psi	Percentage p	Ratios of v_u/f_c'	Ratios of Vd/M
IV-1	3920	4.76	1.00	1.00
I-6b	3570	4.76	1.90	1.67
II-20b	2960	4.25	2.70	2.84
B-10	3470	1.89	1.00	1.00
B-1	3130	1.63	1.17	1.14
III-24b	2990	2.72	3.07	2.28

The data in the last two columns *indicate* proportionality between Vd/M and v_u/f_c'. But v_u was computed from the test data as $8V_u/7bd$; accordingly, the ultimate load P_u *appears to be* proportional to Vd/M and the ultimate shear moment M_u *appears to be* independent of the magnitude of shear. These same conclusions cannot be reached easily by comparing beams of widely different concrete strengths and percentages of longitudinal reinforcement because the factor of proportionality is a function of both f_c' and p. Beams B-9, A4, and III-27a, selected by Dr. Taub, have widely different strengths of concrete, so that they are not suitable for comparisons of the type shown above.

Dr. Taub's calculation of the forces in vertical stirrups is based on several inaccurate assumptions. In the first place, the equation $T_s^v = vs/jd$ is correct only if all of the shear is carried by the stirrups and if the diagonal crack is inclined at 45 deg. It is fairly obvious that the compression zone of concrete transfers a portion of shear. The test observations suggest that the inclination of the crack decreases with increasing a/d ratio; thus for the majority of the beams the inclination of the major diagonal cracks should be less than 45 deg with the beam's axis. Furthermore, the moment arms for the forces T_1 and T_2 shown in Fig. N are not the same, because a portion of the compressive forces at section 2-2 is transmitted under the diagonal crack. All three assumptions—the transfer of full shear by the stirrups, the inclination of the diagonal crack, and the length of the moment arm of forces T_1 and T_2—are considerably in error, thus rendering the resulting equations of questionable value. This conclusion is supported by test observations: had the horizontal force in the stirrups been as large as the vertical force, some vertical cracks would have had to form along the stirrups as a result of dowel action. No such cracks were observed in the tests.

The authors wish to thank Messrs. Ruble and Taub for their valuable discussions, and to amplify Mr. Ruble's suggestion of the need for simplifications for design purposes and Dr. Taub's suggestion of horizontal web reinforcement. Both of these suggestions point out subjects which deserve exploration in the near future.

Discussion of a paper by A. E. Cummings:

Strength Variations in Ready-Mixed Concrete*

By VALENS JONES, OLIVER G. JULIAN, JOE L. McKINSTRY, EDMUND A. PRATT, WALTER K. WAGNER, STANTON WALKER and D. L. BLOEM, and T. F. WILLIS and L. T. MURRAY

By VALENS JONES†

It is a pleasure to know that more engineers are using statistical tools to make comparisons of one job with another and to investigate variations on the job. Standard deviation and coefficient of variation have been used by the Bureau of Reclamation since 1942 for three purposes: investigating variability on the job, summarizing report data from field jobs, and analyzing data by means of control charts.[1] This last item prompted the discussor to write what is intended as an addition to the author's paper. It is hoped that the discussion will induce others to apply control chart analysis to their data so as to have a recognized basis for taking action when test results show variations beyond those attributal to chance.[2]

Concerning the author's analysis, there is statistical background for selecting one rather than several cylinders from a batch. For example, suppose the coefficient of variation (C.V.) of three cylinders from a batch is 5 percent and the averages from batch to batch have a C.V. of 15 percent. Then the job C.V. will be the square root of 5^2 plus 15^2 which equals 15.8 percent. Hence, small within-the-batch variation adds but little to the total. However, the discussor wishes to point out that all of the causes of variability mentioned in the author's category (a) are included and form an integral part of category (b). Ruettgers and Wing[3] list many items causing variation in concrete strength. Wing, Jones, and Kennedy[4] show that wide differences in strength can result from insufficient mixing.

With a competent laboratory employed, as in the case with data presented by the author, it can be assumed justifiably that variations due to category (a) are for the most part small. However, testing laboratories often receive field cylinders which indicate that little care was taken in the fabrication, storage at the job, and transit to the laboratory.

CONTROL CHART METHOD OF ANALYSIS

The method of analysis employed by the author requires waiting until the job is done before the evaluation is made. The "control chart" method

*ACI JOURNAL, Apr. 1955, *Proc.* V. 51, p. 765. Disc. 51-38 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

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makes possible a continuous evaluation as test data are received and in addition provides a criterion which points out the cases where an investigation of causes of variation may be fruitful.

Control charts may be constructed in several ways depending on the manner in which the data were taken and the use to which the analysis is put.[2] The discussor arbitrarily chose one of the simple methods which can be computed readily with an adding machine and slide rule. Basically, the control chart is constructed on the hypothesis, substantiated in practice, that variations within test groups that exceed three times the job standard deviation, σ, are due to causes that can be identified. Conversely, variations less than 3σ are normal for the control of quality on the job and time will be wasted trying to establish causes for these variations.

Range, R, the difference between the maximum and minimum value in a group of tests, is a fairly efficient means of estimating σ for small groups. A control chart could be constructed for each half-day's work or for each day. Since R is not recommended for large groups, the discussor arbitrarily chose a group size of four which is evenly divisible into the total number of tests. Construction of the control chart involves only the following simple steps: The average for each consecutive set of four test cylinders was computed together with the range of strength within each group. These data are plotted as circles in the upper and lower part of Fig. A. Next a running average of the groups, $\overline{X}$, and of the ranges, $\overline{R}$, was computed. Control limits for the group averages and for the ranges are given by the formulas

$$\overline{X} \pm A_2\overline{R} \quad \text{and} \quad D_4\overline{R}$$

where A_2 and D_4 are taken from Table 1, reference 2. The lower limit of range is zero for four tests in a group. Numerically these factors are, respectively, 0.729 and 2.282 for test groups including four cylinders. For fewer tests in a group, these factors are larger and for more in a group they are smaller. The dotted lines in Fig. A show the consecutive control limits obtained by considering all the data to the left of each group average. The final control limits are the heavy dashed lines.

DISCUSSION OF CONTROL CHART

Observing the chart for R at the bottom of Fig. A, only one range, group 23, is outside of control limits. This group includes the first two tests on June 1 in Table 1 of the author's paper. Fig. 1 shows that such variations are to be expected in a group of 208 tests with σ of 454 psi. However, the control chart indicates that an investigation into the physical conditions and supplementary data, including mix proportions, curing, and testing, would disclose an assignable cause for the variation. Statistically, the chances that an assignable cause exists are 99.73 out of 100. Practically, since the cause may involve an admission on the part of some one, the chances of finding the cause are less than the theoretical value. On the other hand, you may actually get the culmination of circumstances represented by 0.07 out of 100.

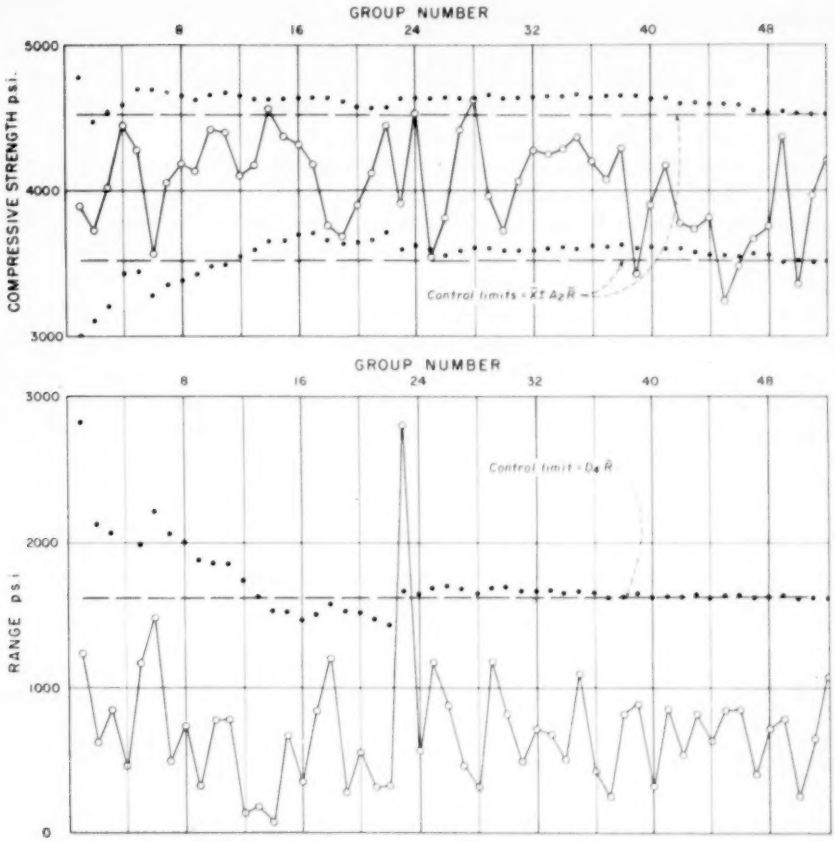

Fig. A—Control chart for averages of four cylinders shows variations that should be investigated

In the control chart for averages at the top of Fig. A, we find no circles outside of the dotted control limits until group 25. The range of strength for this group is 1167 psi with a low strength of 2720 and a high of 3887. The low value would be suspected, and data such as slump, cement content, air content, and defective fabrication would probably assign a cause for the low strength. If in a critical member, it might be necessary to provide additional curing, to core drill as a further check, or to remove the defective concrete to insure safety of the structure.

Group 28 is right on the upper control limit and R is only 318 psi. While the architect and engineer might not be worried by the high strength, the concrete producer might do well to check the yield per cubic yard to see if the cement content is actually in accordance with specifications. Mill tests on the cement might also furnish a clue to the high average strength.

There are four additional low groups, numbered 39, 45, 46, and 50, which

are below the lower control limit. All of the causes mentioned above and in the author's categories (a) and (b) might be considered in investigating these groups. None of them have high values of R, hence the low averages may be part of a downward trend due to high temperatures which raise the water requirement for a workable consistency.

The heavy dashed lines represent the final control limits applicable to the job. They do not materially change the decisions made earlier. It will be remembered that the 3σ limits are somewhat arbitrary but have been proved useful. A value near a control line may or may not have an assignable cause for its position.

If it could be determined that any particular test cylinder was defective and not applicable to the concrete in the structure, it would be justifiable to eliminate the strength result from the average. With detailed data at hand, one might be able to assign causes of variation to specific cases. The control chart simply points the way for further investigation. It is hoped that others may follow the author's lead in presenting data for the benefit of the concrete industry.

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By OLIVER G. JULIAN*

Mr. Cummings has presented an excellent, clear, and concise paper on a much neglected subject. All too few engineers realize the magnitude and relative frequencies of the variations in strength and other properties of the materials which they use. This appears to be because in spite of the great mass of literature, including text and reference books published on properties of materials, the scarcity of data regarding variations in strength and other properties in such form as can be used in statistical or probability studies is remarkable. The references listed at the end of this discussion include a few sources which give data pertaining to concrete. The writer inclines to the view that a text or paper having to do with the properties of materials is indeed incomplete if it does not include such data.

It may be of interest to compare the author's findings with those of the writer for a comparable grade of concrete, *i.e.*, concrete having a mean compressive strength approximating 5200 psi at 28 days with a coefficient of variation approximating 10 percent.

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Fig. B summarizes the results of 861 standard tests representing the 28-day strength of 33,300 cu yd of concrete used on three important jobs during a period of several years, including all seasons of the year. In this figure the rectangular cells, drawn in dots, extending above the axis of abscissas represent the histogram of the test results. The width of these cells has been taken as 0.6745 of a standard deviation of the compressive strength or 0.6745 of the coefficient of variation in percentage deviation from the median. (This width was chosen because it corresponds to a probable deviation of a Gauss-Laplace distribution.) The figures shown in circles indicate the number of test results pertaining to each cell; the corresponding relative frequencies in percentages are given as ordinates. The curve drawn in solid lines represents the probability of occurrence. The ordinates pertaining to this curve indicate (1) the percentage of total number of samples testing below the value indicated by the corresponding abscissa to the left of the median, and (2) the percentage of total number of samples testing above the value indicated by the corresponding abscissa to the right of the median. Pertinent figures regarding this set of tests are as follows:

Standard deviation	540 psi
Coefficient of variation	10.4 percent
Highest test	6980 psi
Average	5180 psi
Lowest	3810 psi

The strength f_c' used in design of the structures on which this concrete was placed was 3825 psi. However, the control of the concrete was aimed at a minimum strength of 4500 psi. It will be noted that approximately 10 percent of the test results fell below this latter figure which is midway between the median and the lowest test. The writer has found such figures to be typical for cases in which special care is taken as to control. The data given above were previously presented in reference 2.

Although the histogram representing the relative frequency in Fig. B appears quite irregular, a comparison of the probability curve (drawn in heavy lines) with the Gauss-Laplace ogive indicates that they are practically identical except for extreme values (the tails of the curves). As has been indicated by Freudenthal[1] (see his Fig. 8 which was made from the same data as the writer's Fig. B, less 188 typical tests), the extreme values correspond much better with the log-normal than with the normal Gauss-Laplace distribution. Also, the body of the curve above the 10 percent ordinate corresponds approximately as well with the log-normal as with the normal distribution. In sum, the log-normal distribution fits the tests closer than the normal distribution and the former should be used in case extreme values are of importance.

The agreement in findings by the author, Bloem,[5] and the writer appears to be remarkably close. This is most gratifying. However, in addition to representing a greater number of tests, it should be noted that the writer's

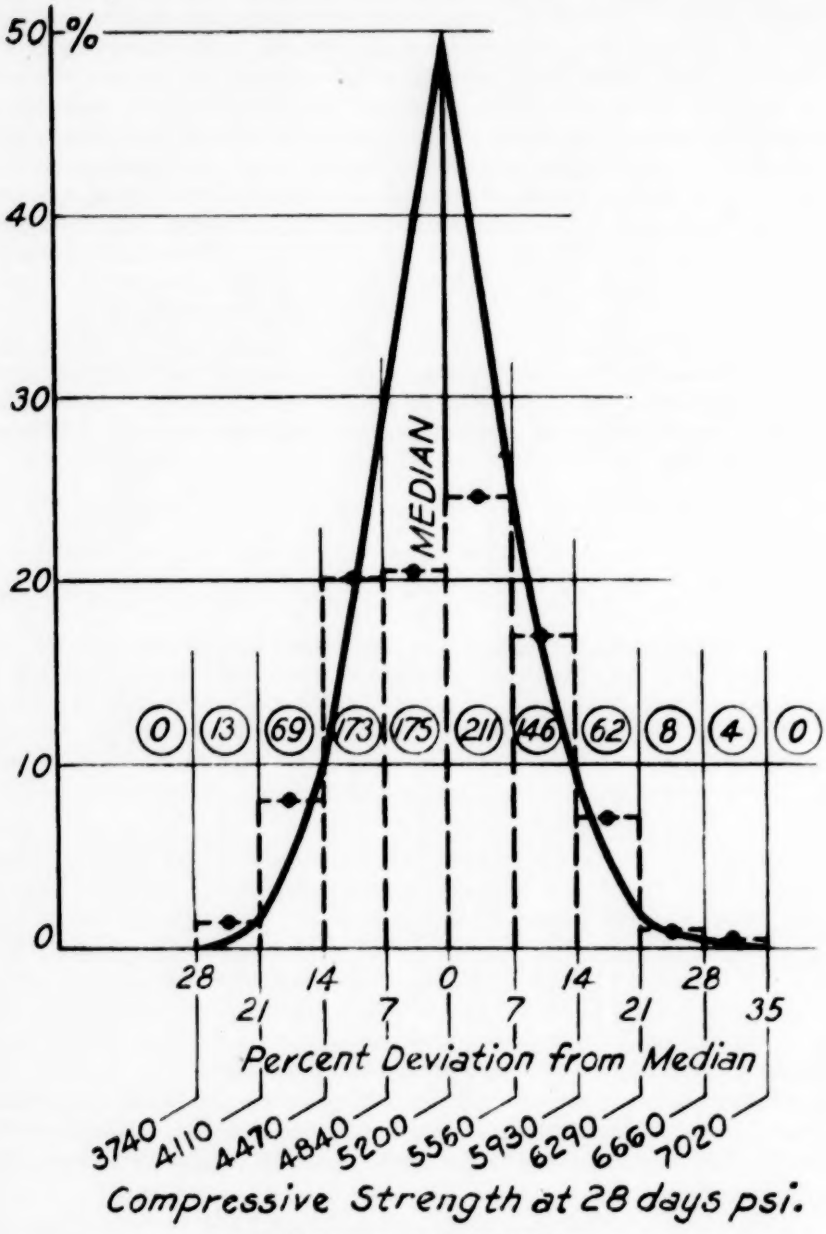

Fig. B—Summary of 861 standard tests

findings are based on the following listed conditions which are quite different from those on which the author's and Bloem's[5] findings are based.

1. Several brands of Type II cement were used.
2. Graduation of the fine aggregate was at times faulty.
3. Maximum size of the gravel was varied with placement conditions from ⅜ to 1½ in.
4. The water-cement content was varied between 4¼ and 5½ gal. per sack of cement, dependent upon the aggregate and cement used and placement conditions.
5. The cement factor was varied between 658 lb (7 sacks) and 940 lb (10 sacks) per cu yd of concrete, dependent upon placing conditions, the desired degree of workability, and the aggregate and cement used.

The above variations were necessary to utilize available materials, obtain the workability desirable for a wide range of placing conditions, and at the same time maintain the spread in strength within reasonable limits and fairly consistently above 4500 psi.

Thompson & Lichtner Co., Inc., were in charge of concrete control. The concrete was furnished by J. P. O'Connell Co. and placed by the general contractor, Thomas O'Connor & Co., Inc. The record is believed to indicate a most creditable performance.

The writer believes it advisable to emphasize that unless the supervising personnel is constantly on the job, vigilant and efficient, the variations indicated by the author, Bloem,[5*] and the writer may be exceeded by 150 percent or even more. For one job with which the writer is acquainted and which was difficult to control, a summary of 296, 28-day tests representing 3300 cu yd of concrete is as follows:

Standard deviation	1540 psi
Coefficient of variation	25.6 percent
Highest test	9100 psi
Median	6000 psi
Average	5850 psi
Lowest	1500 psi
Desired minimum	4300 psi
Percent of tests below desired minimum	17 percent

A plot of these data on extremal probability paper has been presented by Freudenthal.[1] Cases such as this may be more nearly typical of common practice than the case represented by Fig. B. Reference 5, Table IV, shows coefficient of variation ranging from 11.0 to 21.8 percent at 28 days on 10 jobs; in eight cases the coefficient is 15 percent or higher.

The author's Fig. 3 indicating daily strength variations is most interesting especially since all of the concrete was placed during what would appear to be an ideal season, May 25 to June 12. One might with considerable justification question the reasons for the "freak" tests mentioned. Could it be that the control engineer was absent for a moment or two?

The freak tests point out the danger of drawing conclusions from only a few tests. It will be recalled† that a more precise expression for the standard deviation than that given by the author as Eq. (1) is:

*For a well-controlled job. See Bloem's Fig. 2.
†See *Handbook of Engineering Fundamentals*, 2nd Edition, 1952, edited by O. W. Eshbach, John Wiley & Sons, Inc., New York, p. 2-33.

$$\sigma = \sqrt{\frac{\sum_{1}^{N} (X - \bar{X})^2}{N - 1}} \quad \text{.....(a)}$$

In case $N = 1$, this expression states that $\sigma = 0/0$, an indeterminate. However, Eq. (a) approaches the author's Eq. (1) rapidly as N increases. The difference for $N = 208$, the number of tests used by the author, is negligible.

The author states: "When the engineer can be sure of getting concrete with a high degree of uniformity in quality and strength, he can design with reduced safety factors or lower the cement factor, and he can expect reduced maintenance costs on structures exposed to weathering." This cannot be emphasized too strongly. Another way of expressing this thought would be: A comparatively small sum expended on concrete control will ordinarily pay large dividends both in first cost and in maintenance costs.

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By JOE L. McKINSTRY†

Other factors should be considered when using the classification system established by the Danish Institute of Building Research. When using the coefficient of variation as a measure of quality control it should be remembered that V is only reliable when used in the presence of the standard deviation and the arithmetic mean. When abstracted from them it could be misleading.

It should also be realized that an increase in the coefficient of variation could result from an increase in the standard deviation or a decrease in the arithmetic mean. Therefore quality control cannot be measured by a "percentage yardstick" alone, but concomitant with other methods.

*The ordinates in figures noted in references 6, 7, and 8 have the same meaning as those in Fig. B of this discussion.

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By E. A. PRATT*

This paper gives further evidence that wider use of statistical methods in the study of test results of standard concrete compression test cylinders can contribute much to progress in improving the quality of concrete and its day to day control. Any procedure that simplifies the analysis, or makes it more effective, is therefore desirable.

The purpose of the following discussion is twofold: (1) to show how the arithmetical work of this and similar studies can be greatly reduced without impairing its accuracy, and (2) to show how the application of more advanced statistical methods can extract from the same data considerably more information, and this of such a nature that the author's estimate of the quality of the concrete may have to be revised.

SIMPLIFICATION OF THE ARITHMETIC

Cursory examination of Tables 1 and 2 discloses that the compressive strengths given in psi are derived from total load figures taken to the nearest 1000 lb on 6-in. diameter cylinders. The psi figures can, therefore, be accurate only to the nearest 17.7 psi and conversion to units of 1 psi gives the appearance of a degree of accuracy that does not exist. Incidentally, ASTM Designation C 39, "Standard Method of Test for Compressive Strength of Molded Concrete Cylinders," calls for calculation of compressive strength to the nearest 10 psi. This would require the total load on a 6-in. cylinder to be taken to the nearest 500 lb.

Use of compression strength figures in the original 1000-lb units throughout the statistical analysis has distinct advantages. It maintains the full accuracy of the work and at the same time greatly reduces the arithmetical labor. Only two- or three-digit figures are involved and the first digit of the three-digit figures is always 1. Table A, showing the 7-day strengths for May 25, illustrates this point. Obviously, it is much easier to manipulate the two- and three-digit figures of the right-hand column than the four-digit figures from Table 1. Final conversion of the results of the analysis into psi is a simple matter, as will appear later.

Since evaluation of the results of the analysis requires the preparation of frequency distributions, such as those shown in Fig. 1 and 2, a further saving in arithmetical labor can be gained by preparing these distributions first and using them in the computation of average strength and standard deviation, instead of working through the long tabulations of Tables 1 and 2. Table B shows the frequency distribution of 7-day strengths using a class interval of four 1000-lb units of load. Note that the midpoints of the 4000-lb intervals have been so chosen that no entry can fall on an interval boundary. This in no way affects the result of the analysis, but it simplifies the computations.

The simplicity of carrying out the analysis on 19 one- and two-digit figures, instead of on 208 four-digit numbers, needs no emphasis. Indeed, it is not

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TABLE A—7-DAY STRENGTHS FOR MAY 25

Strength from Table 1, psi	Total load, lb × 10^{-3}
4346	123
3852	109
4240	120
3109	88
3887	110
3852	109
3263	92
3781	107

TABLE B—7-DAY STRENGTH FREQUENCIES

Interval midpoints	Frequency	Interval midpoints	Frequency
146.5	1	106.5	21
142.5	–	102.5	7
138.5	3	98.5	13
134.5	5	94.5	9
130.5	15	90.5	4
126.5	20	86.5	4
122.5	22	82.5	1
118.5	25	78.5	2
114.5	30	74.5	1
110.5	24	70.5	1

even necessary to use the three- and four-digit numbers representing the interval midpoints in the calculations. By taking some midpoint (*e.g.*, 114.5, the highest frequency) as a working mean and numbering the intervals 1, 2, 3, etc., in both directions from this mean the numbers representing the midpoints will range from −11 to +8 (see Table C). These are the numbers that have to be squared, instead of four-digit psi figures.

The simple procedures for the analysis of such a frequency distribution need not be described here; they may be found in reference 1, Chapter III, and in other works on statistical methods.

In the case of the 7-day strengths it is found that the true mean is −0.207 interval from the working mean of 114.5, making its value 113.67 units of 1000 lb, or exactly 4017 psi, as found by the more laborious procedures followed in the paper. The standard deviation is found to be 13.08 units or 462 psi, as compared with 464 psi found by the author. It is not ordinarily necessary to take into account the error introduced by grouping the observations. In the present case the error of the mean is too small to be apparent and the difference in standard deviation is only 2 psi, an insignificant figure.

From the foregoing it is evident that considerable labor can be saved by using for the statistical analysis the original test results before conversion into psi, and even more by carrying out the analysis on the frequency distribution rather than on the individual strength figures.

ANALYSIS OF VARIANCE

Fig. 3 shows that there is wide variation in test results from day to day, but Fig. 1 and 2 suggest that taken all together the frequencies, as shown in the histograms, tend to resemble the normal curve of error. *This over-all averaging has the effect of concealing the strength variations that are at the heart of the problem of concrete quality control.*

The data of the paper are not sufficient to permit an analysis that would pin-point the several sources of variations. They are, however, sufficient to permit determination of relative importance of variations *between* days as compared with variations between batches made on individual days. (The latter will be referred to as variations *within* days.) This result is accom-

TABLE C—ANALYSIS OF 7-DAY STRENGTHS

Interval midpoints	Deviation d	Frequencies (f) by days													
		5/25	5/26	5/27	5/28	5/29	6/1	6/3	6/4	6/9	6/10	6/11	6/12	Σf	$d^2\Sigma f$
146.5	8						1							1	64
142.5	7													—	—
138.5	6			1			1	1						3	108
134.5	5		1	1	1			1		1				5	125
130.5	4		2	3	4	1	1	1	2	1				15	240
126.5	3		2	1	3	2		3	3	3	2		1	20	180
122.5	2	1	1	2	4	3	2	3	2	1			3	22	88
118.5	1	1	4	2	5	1		2	5	3	2			25	25
114.5	0		2	9	4	5		1	2	3	2	1	1	30	0
110.5	−1	3		3		3	1	2	2	1	6	2	1	24	24
106.5	−2	1			1	2	4	1	1	2	4	5		21	84
102.5	−3		1			1					2	2	1	7	63
98.5	−4		2			2	1				4	3	1	13	208
94.5	−5					2		1				5	1	9	225
90.5	−6	1						1		1		1		4	144
86.5	−7	1									1	2		4	196
82.5	−8											1		1	64
78.5	−9						1					1		2	162
74.5	−10		1											1	100
70.5	−11						1							1	121
Totals (T)		8	16	22	22	22	13	17	17	16	23	23	9	208	2221
Σfd		−15	4	29	41	−11	−11	17	22	12	−35	−92	−4	(N) −43	
$(\Sigma fd)^2/T$		28.12	1.00	38.23	76.41	5.50	9.31	17.00	28.47	9.00	53.26	368.00	1.78		

$\Sigma[(\Sigma fd)^2/T] = 636.08$ Correction for mean $= (\Sigma\Sigma fd)^2/N = (-43)^2/208 = 8.89$

Total sum of squares $= 2221.00 - 8.89 = 2212.11$

Sum of squares between days $= 636.08 - 8.89 = 627.19$

Sum of squares within days $= 2212.11 - 627.19 = 1584.92$

plished by application of the procedure known as the analysis of variance, details of which can be found in references 2, 3, and 4.

Table C shows the frequency distributions of the 7-day strengths for individual days, the total frequency distribution (*i.e.*, the row totals), and other products necessary for the computation of variances. Numerical results of the analysis are summarized in Table D.

Before discussing the statistical analysis it is worth while to note the way in which Table C shows how the different days contribute to the over-all frequency distribution (Fig. 1). It is seen that the contributions of May 27 and 28 are almost entirely to the right-hand side of Fig. 1 (*i.e.*, above the average), while June 11 contributes only to the left-hand side. May 29 is fairly evenly balanced between left and right, while June 1 (equally balanced but with fewer batches) is the only day on which both extremes of strength were found, the spread being the full width of the frequency diagram. These

TABLE D—ANALYSIS OF VARIANCE

Source of variation	Sums of squares	Degrees of freedom	Mean squares (variance)	Standard deviation		Coefficient of variation, percent
				Intervals	psi	
Between days	627.19	11	57.02	7.551	1067	26.57
Within days	1584.92	196	8.09	2.844	402	10.01
Total	2212.11	207	10.69	3.270	462	11.51

Variance ratio, $F = 57.02/8.09 = 7.05$

F—test probability, $P < 0.005$

facts are also suggested by Fig. 3 of the paper, though the relative frequencies of Table C make them much more apparent, giving weights, as it were, to the different strengths.

Separation of the variances (*i.e.*, the mean squares of deviations) into the portion contributed by variations between days and the portion coming from within-day variations requires computation of the total sum of squares and the sums of squares for the respective portions. Since a working mean (114.5 units) instead of the exact mean has been used it is necessary to make a "correction for the mean" when computing the sums of squares; this is the figure 8.89 that has been subtracted from the crude sum of squares in each case. The sum of squares within days is merely the difference between the total sum of squares and the sum of squares between days.

It will be noted that "degrees of freedom" are used in computing the mean squares. The statistical theory underlying the use of degrees of freedom instead of number of observations, or number of groups, as the case may be, when computing the mean square and standard deviation is somewhat involved, but it is essential to take it into consideration in cases where comparatively small numbers of observations are used; otherwise, inaccuracies may be introduced (see reference 3, Chapter 3).

The analysis of variance is a means of determining whether the variations in strength from day to day are due to the same causes that produce the variations between batches made on the same day. If the same causes produce the within- and between-day variations, then the standard deviations derived from the two different mean squares (variances) are estimates of the same "standard error" and should not differ greatly. If there is a wide difference between them it shows that different causes are involved in the within- and between-day variations. The ratio of the variances is the index from which we can calculate the probability that the causes are not the same.

In the case of the 7-day strengths the variance ratio (usually symbolized by F) is 7.05, as shown at the bottom of Table D. Tables and charts of F are to be found in references 2, 3, and 4, and from these it is found that for a value of $F = 7$ and the degrees of freedom involved the probability of the within- and between-day variations being produced by the same causes is less than 0.005. In other words, the odds are more than 200 to 1 that the between-day variations come from a different set of causes than those which produce the within-day variations. Of course, these two sets of causes need not be mutually exclusive; they may have common elements.

The analysis of variance shows, moreover, that correction of the between-day variations—that is, eliminating variations other than those due to errors normally to be expected from batch to batch—would greatly reduce the overall variability of the concrete. Expressed in figures: the author finds a standard deviation of 464 psi with a resultant coefficient of variation of 11.55 percent, but the analysis of variance discloses that the standard deviation considering only the variations from batch to batch (within days) is 402 psi, while that derived from variations between days is 1067 psi. These standard

deviations give coefficients of variation of 10.0 and 26.6 percent, respectively. In the classification of Table 3, the quality measured within days is "excellent" while that measured between days is "bad."

In searching for an explanation of the day to day variation one might well ask: What happened on June 4 to cause the concrete to be of such highly consistent quality? And can these conditions be repeated? Both the 7- and 28-day strengths for this date are closely grouped about mean values, that for 7 days being 5 percent above the general 7-day average and that for 28 days 1 percent under the general 28-day average. Considering this day alone, the coefficients of variation are 6.06 and 4.34 percent for the 7- and 28-day strengths, respectively. In the words of Table 3, these values are "attainable only in well controlled laboratory work." It seems reasonable to hope that further studies will reveal causes of variation that can be eliminated, so that eventually such values may be attainable in well controlled plant work. Vast economies in the use of concrete could be achieved this way. In contrast with the low coefficients of variation for June 4 are the values for June 1, *viz.*, 20.05 and 10.89 percent for the 7- and 28-day strengths, respectively—roughly three times as great as the corresponding values for June 4.

In the foregoing discussion only the analysis of the 7-day strengths has been described. The same analysis was carried out for the 28-day samples with the following results:

Variance ratio: 7.17	
Standard deviation, over-all:	492 psi
Standard deviation, within days:	427 psi
Standard deviation, between days:	1143 psi
Coefficient of variation, over-all:	9.36 percent
Coefficient of variation, within days:	8.12 percent
Coefficient of variation, between days:	21.76 percent

The same pattern is seen to prevail in the case of the 28-day strengths, though the coefficients of variation are somewhat lower than for 7 days. But the variation *between* days remains about 2½ times that *within* days.

Determination of the relative importance of the many causes of variation in concrete quality will obviously require planned experimentation. This does not necessarily mean special series of laboratory tests; it can be made a part of routine testing. Much can be learned from adequate plant and laboratory records without increasing the number of samples tested, but the system of records must be designed with a view to subsequent statistical analysis. The final objective is, of course, scientific and practical day to day quality control.

As a postscript to this discussion it may be useful to emphasize the importance of presenting original data in detail, as the author did in Tables 1 and 2. The Institute's publications policy as to the avoidance of extensive test data and the use of "average values," while in general perfectly sound, would in papers of this kind often result in concealment of the very factors

of the problem that are being sought. This is apparent from the discussion in connection with Table C.

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By WALTER K. WAGNER*

Mr. Cummings has presented an interesting and instructive analysis of the test data for ready-mixed concrete placed on one project. Although the method of statistical analysis is credited to the Danish National Institute of Building Research, the same method was presented to an assembled group of the National Ready Mixed Concrete Assn. by Stanton Walker on Jan. 27, 1944.[1] It has been circulated freely in more recent publications of that group.[2]

The close relationship of actual to theoretical distribution of test values as presented by Cummings and, in a somewhat different but more precise pattern, by Bloem[3] demonstrates the usefulness of this "tool" in more scientific control of a ready-mixed concrete operation. There is a definite danger, however, in using the values of one group of tests as the criterion for evaluating the complete operation.

Table E gives a comparison of laboratory and field tests for a fairly large group of mixes. Even under laboratory control the coefficient of variation is not a stable value (group 1 through 9). A consistently lower coefficient was obtained for cement brand B than for brand A. Thus, cement from different mills cannot be assumed to give the same results. If the brands must be mixed, as is often the case, then the design must be based on the cement which produces the widest variation.

The field tests (group 9 through 17, Table E) show coefficients of variation from 5.8 to 16.2 percent. It would be impractical to select one value for a working coefficient of variation that would assure an economical design and still stay within a reasonable limit of low tests.

Table F gives a comparison of actual failures to theoretical failures determined from the coefficient of variation. This shows an even closer agreement than the data presented by Bloem.[3] Groups 16 and 17 have unusually low coefficients of variation with no failures reported, even though, theoretically, there should be some low tests. This could be due to the small number of tests reported and the time of delivery which occurred during the season when cement quality is generally better than average.

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TABLE E—COMPARISON OF LABORATORY AND FIELD STRENGTH TESTS OF CONCRETE

Group No.	Cement brand	W/C by weight	7-days					28-days				
			Number of tests	Number of cylinders	Average strength, psi	Standard deviation, psi	Coefficient variation, percent	Number of tests	Number of cylinders	Average strength, psi	Standard deviation, psi	Coefficient variation, percent
Laboratory tests												
1	A	0.88	121	242	1620	231	14.2	121	242	2770	340	12.3
2	B	0.88	89	178	1680	130	7.7	89	178	2820	200	7.1
3	A	0.88	34	68	1730	166	9.6	34	68	2850	274	9.6
4	B	0.88	33	66	1730	154	8.9	33	66	2750	234	8.4
5	A	0.65	34	68	2940	298	10.1	34	68	4370	412	9.5
6	B	0.65	33	66	2850	255	9.0	33	66	4230	334	7.9
7	A	0.65	56	59	2900	399	13.7	49	140	3840	481	12.5
8	B	0.65	83	87	2780	273	9.8	83	259	3990	360	9.0
Field tests												
9		0.81	51	54	1830	302	16.5	51	130	2830	404	14.3
10		0.77	44	46	1980	363	18.4	44	118	2920	462	15.8
11		0.71	153	153	2470	318	12.9	153	153	3780	408	10.8
12		0.71	85	87	2350	475	20.2	87	220	3390	551	16.2
13		0.69	80	80	2660	433	16.3	81	82	3670	448	12.2
14		0.69	89	89	2390	364	15.3	89	89	3500	363	10.4
15		0.68	62	62	2330	339	14.6	61	164	3280	478	14.6
16		0.59	19	19	3660	370	10.1	19	57	4700	303	6.4
17		0.44	23	23	4930	573	11.6	23	69	5960	346	5.8

TABLE F—COMPARISON OF ACTUAL TO THEORETICAL FAILURES

Group No.	Specified 28-day strength, psi	7-day strength, psi			28-day strength, psi			Coefficient variation, percent		Percent failures at 28 days	
		Average 5 high	Average 5 low	Average	Average 5 high	Average 5 low	Average	7-day	28-day	Actual	Theoretical
9	2500	2300	1270	1830	3530	2130	2830	16.5	14.5	20	21
10	2500	2690	1460	1980	3640	2220	2920	18.4	15.8	18	18
11	3000	3660	1830	2470	4650	2870	3780	12.9	10.8	2	2
12	3000	3540	1490	2350	4610	2340	3390	20.2	16.2	25	22
13	3000	3750	2050	2660	4680	2910	3670	16.3	12.2	4	6
14	3000	3230	1760	2390	4430	2890	3500	15.3	10.4	5	8
15	3000	2980	1640	2330	4090	2450	3280	14.6	14.6	23	27
16	3750	4020	3220	3660	5070	4360	4700	10.1	6.4	0	2
17	5000	5490	3980	4930	6420	5530	5960	11.6	5.8	0	8

When concrete of a given quality is delivered to several contractors whose opinions on handling, placing, and especially testing differ widely from each other, then it can be expected that wide variations will occur in the final test results. This can be understood readily when the methods described in Table G are considered with the final results of the test data. Groups 9, 10, 12 and 15 represent concrete deliveries to a fairly large group of contractors, some of whom elected to make and cure their own test cylinders in an effort to save a few dollars on their testing requirements. As a result of this mishandling a controversy arises between architect, contractor, and supplier as to the adequacy of the concrete. In many cases the architect places a blind faith in a test report without ever questioning the sampling and testing or the possibilities that water was added at the job prior to obtaining samples.

Test groups 11, 13, 14, 16, and 17, were obtained from jobs that had supervision of placement and in the case of group 11, the batching as well. All tests were made by a commercial laboratory. A maximum coefficient of variation of 12.2 percent would place the control under the classification of "good" to "excellent."

In a more practical sense this means that mixes to supply the general trade must be overdesigned about 60 percent to have 99 chances in 100 of meeting specification strength; whereas, for a particular project, with proper understanding between all parties, the overdesign would not have to exceed 40 percent—or for concrete designed for 3000 psi there would be a difference of about 0.9 bag of cement.

The value of proper control for a ready-mixed concrete operation cannot be placed too high, but the sincere cooperation of everyone connected with any part of the end result is necessary if that control is to be really worthwhile.

REFERENCES

1. Walker, Stanton, "Application of Theory of Probability to Design of Concrete for Strength Specifications," Draft for presentation before 14th annual meeting of National Ready Mixed Concrete Assn., Jan. 27, 1944.

2. "Control of Quality of Ready-Mixed Concrete," *Publication* No. 44, 3rd Edition, National Ready Mixed Concrete Assn., Washington, D. C., 1953. (See chapter on "Over-Design to Meet Specifications.")

TABLE G—DESCRIPTION OF CONCRETE MIXES AND METHODS OF BATCHING AND TESTING FOR TESTS LISTED IN TABLES E AND F

Group No.	Description
1	Laboratory pilot mix data obtained from standardized mix of 1:3.72:3.91 with ¾-in. aggregate and W/C = 0.88. Tests were made on single car shipments of cement of one brand (brand A) received between September, 1949, and August, 1950. Tests represent 177 cars received. Group includes tests of 5-car composite samples for a short period. Tests are average of two 6 x 12-in. cylinders for both 7- and 28-day tests. Batches were mixed under careful laboratory control for 5 min in a 2-cu ft tilting mixer. Cylinders were moist cured at 70 F.
2	Same as group No. 1 except for a different brand of cement (brand B). Tests represent 138 cars received between September, 1949, and August, 1950.
3	This group represents the last 34 tests of group No. 1, cement brand A.
4	Represents last 33 tests of group No. 2, cement brand B.
5	Laboratory pilot mix on cement represented by group No. 3 tests except that a standard mix of 1:2.48:3.72 with ¾-in. aggregate was used. W/C was 0.65 by weight. Procedures for mixing and testing all pilot mixes were as described for group No. 1 tests.
6	Laboratory pilot mix of 1:2.48:3.72 with W/C = 0.65 and ¾-in. aggregate (as in group No. 5). Cement (brand B) was from the same samples as used for group No. 4 tests.
7	Laboratory pilot mix of 1:2.48:3.72, ¾-in. aggregate, W/C = 0.65. This group was tested between December, 1954, and June, 1955, using samples of cement brand A received during that period (56 cars tested).
8	Similar to group No. 7 tests except that data represents 83 cars of cement brand B.
9	Ready-mixed concrete supplied for general construction work to several contractors from April, 1954, to June, 1955. Design strength, 2500 psi; maximum aggregate, ¾ in. All cylinders were tested and reported by a single commercial testing laboratory; however, in some cases, contractor elected to make his own cylinders and job cure them.
10	Ready-mixed concrete for general construction batched during the same period and sampled and tested under conditions similar to those for group No. 9. Design strength, 2500 psi; maximum aggregate, 1½ in.
11	Ready-mixed concrete delivered to a single government building project from November, 1949, to September, 1950. Design strength, 3000 psi; ¾-in. aggregate. Batching and testing were controlled by commercial laboratory. Placing was under supervision of government inspectors.
12	Ready-mixed concrete supplied to several contractors for general construction from April, 1954, to June, 1955. Conditions of testing similar to those described for group No. 9. Maximum aggregate, ¾ in.; design strength, 3000 psi at 28 days.
13	Ready-mixed concrete delivered to one contractor for curb and gutter from January to December, 1951. Maximum aggregate, 1½ in., design strength, 3000 psi at 28 days. All cylinders were molded, cured, and tested by commercial laboratory.
14	Ready-mixed concrete for curb and gutter delivered to three contractors from February to November, 1952. Same mix as group No. 13. Cylinders molded, cured, and tested by commercial laboratory.
15	Ready-mixed concrete for general construction delivered between April, 1954, and June, 1955. Maximum aggregate, 1½ in.; design strength, 3000 psi at 28 days. All cylinders tested and reported by commercial laboratory. Some cylinders made by contractors' personnel and job cured.
16	Ready-mixed concrete delivered to a single reinforced concrete building from September, 1953, to June, 1954. Design strength, 3750 psi at 28 days; ¾-in. maximum aggregate. All cylinders molded, cured, and tested by commercial laboratory. Placement was supervised by representative of architect.
17	Ready-mixed concrete delivered to same project and under same conditions as described for group No. 16. Maximum aggregate ¾ in.; design strength, 5000 psi at 28 days.

3. Bloem, Delmar L., "Studies of Uniformity of Compressive Strength Tests of Ready-Mixed Concrete," *ASTM Bulletin*, No. 206, May 1955, p. 65.

By STANTON WALKER and D. L. BLOEM*

The author has presented data which are pertinent to an important problem of concrete control. These data make clear that, if concrete is to be evaluated realistically on the basis of its strength, account must be taken

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of the variations in measured strength which are inevitable however well the concrete may be controlled. No economic amount of overdesign will insure that not any tests whatever will fall below the specified strength.

Some nonuniformity in measured strength is inherent in concrete production and testing and it results from a combination of real and apparent variations in quality. The degree of nonuniformity depends upon the accuracy with which the concrete is controlled and tested. Specifications for strength which are practicably administrable must assume that some tests may fall below requirements. The amount which average strength must exceed specified strength depends upon the degree to which concrete production and testing are controlled and upon the percentage of tests under specified strength which will be tolerated.

Table 3 expresses degree of control in terms of coefficients of variation. That classification is incorrectly attributed to the Danish National Institute of Building Research. It was developed by the senior author of this discussion and first published in the paper "Application of Theory of Probability to Design of Concrete for Strength Specifications," presented at the 14th annual meeting of the National Ready Mixed Concrete Assn. in January, 1944.* The classification was also included in the paper by Niels Munk Plum "Quality Control of Concrete—Its Rational Basis and Economic Aspects," in the *Proceedings* of the Institution of Civil Engineers, London, and adequate credit to source was given, which the author apparently overlooked.

The original paper also indicated the amount of overdesign necessary to meet various tolerances for the percentage of tests under the specified strength. Table II outlines the requirements for 9 in 10 chances and 99 in 100 chances of no specimen falling below 100, 90, 80, and 70 percent of the specified strength. That table deserves careful scrutiny. It demonstrates, on the one hand, that investment in good control is self-liquidating; and, on the other hand, that, no matter how good the control or amount of overdesign, some tests must be expected to show strengths below those specified.

As pointed out, the nonuniformity results from combinations of real and apparent variations in quality. The 1944 paper cited suggests how these two categories may be evaluated. The over-all coefficient of variation (v) is a function of the coefficient of variation attributable to proportioning and mixing in the field (v_1) and the coefficient indicative of the uniformity within the particular testing laboratory (v_2), thus:

$$v = \sqrt{v_1^2 + v_2^2}$$

Further information on reproducibility of strength tests was presented in a recent paper by the junior author of this discussion, "Studies of Uniformity of Compressive Strength Tests of Ready-Mixed Concrete," *ASTM Bulletin*, No. 206, May 1955. Data from 14 different jobs representing a wide range in degree of control were reported, showing coefficients of variation

*See also "Control of Quality of Ready-Mixed Concrete," *Publication* No. 44, National Ready Mixed Concrete Assn., Washington, D. C.

TABLE H—AVERAGE STRENGTH REQUIRED FOR DIFFERENT CONDITIONS

Coefficient of variation of operation	Average strength for various strengths to be met (All strengths expressed as percent of specified strength.)							
	9 chances in 10				99 chances in 100			
	100	90	80	70	100	90	80	70
5	104	...	...	...	113	102	...	...
10	115	104	...	...	130	117	104	...
12	118	106	...	...	139	125	111	...
15	124	112	100	...	154	138	123	108
18	130	117	104	...	172	155	138	120
20	134	120	107	...	187	168	150	131
25	147	132	118	30	239	215	191	167

ranging from 7.0 to 23.6 percent. The latter figure was for a job of several years ago on which volume batching was used and, if it is eliminated, the range becomes 7.0 to 15.1 percent. The variability of strength tests reported by Mr. Cummings, represented by coefficients of variation of about 9 to 12 percent, falls near the middle of the range reported in the above-mentioned paper and in the "excellent" category of the classification table discussed previously. It appears, therefore, that with moderate attention to control, the coefficient of variation can be expected to remain below about 15 percent. That provides the engineer with a realistic basis for specifying strength and establishing when the concrete is acceptable. For the 15 percent coefficient, the following predictions can be made:

1. To insure 99 in 100 tests being above a specified strength, the average strength must be 54 percent higher.
2. To insure 9 in 10 tests being above a specified strength, the average strength must be 24 percent higher.
3. For a given average strength, about 25 percent of the tests will be below 90 percent of the average, 9 percent below 80 percent, 2 percent below 70 percent, and 0.4 percent below 60 percent.

The important thing to be remembered is that strength tests conform quite accurately to the normal probability law and that it is statistically impossible to set any realistic minimum strength and not permit some limited number of tests to fall below that value.

By T. F. WILLIS and L. T. MURRAY*

Mr. Cummings' paper should be of interest to all concerned with the control of concrete under job conditions. He has demonstrated that the measured compressive strengths of job control specimens tend to follow a "normal" distribution, and has described the statistical methods for calculating estimates of parameters that characterize the population of job concretes, of which his were a sample. These parameter estimates, or statistics, are then used to express in a single figure the effectiveness of the control of the job, thus furnishing a simple basis for comparison with other jobs. His data lend themselves to further statistical analysis with information of interest resulting.

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First it would be of interest to know whether all the variation in compressive strength was due to random, uncontrollable factors or if, perhaps, some of it is due to factors which might be isolated and placed under better control. From the data as presented a possible "day of placing" effect can be explored. Resolution of this possibility is accomplished through an "analysis of variance" by subdividing the total variance into the two components, "between placing days" and "within placing days," and comparing the two. Table I shows the results of such an analysis for 7- and 28-day compressive strengths.

TABLE I—ANALYSIS OF VARIANCE

Source of variation	Sum of squares of deviations	Degrees of freedom	Mean square deviation	Estimate of F	Tabulated F for 1 percent probability
7-day strengths					
Between days	1.255×10^7	11	11.41×10^6	6.93	2.34
Within days	3.228×10^7	196	1.65×10^6		
Total	4.483×10^7	207			
28-day strengths					
Between days	1.457×10^7	11	13.24×10^6	7.36	2.34
Within days	3.532×10^7	196	1.80×10^6		
Total	4.989×10^7	207			

In Table I the value in the penultimate column is the ratio of the two values in the preceding column, and is designated as an estimate of the statistic F.

For the ratio of the variances of any two samples from the same normally distributed body of data (samples being of any specific size), values have been calculated and tabulated* which have certain probabilities of being exceeded. The tabulated values for our analyses are shown in the last column of the table. As for the present case, in the examination of two sub-sets of experimental data, the objective is to determine whether the sub-sets are from the same population, *i.e.*, were both produced with the same group of factors operative on each. If the estimated F exceeds the tabulated F at some chosen, small probability level, it is inferred that the sets are in reality samples from different populations and that the difference between their variances is real and not due merely to sampling variation.

In the present instance, if there is no effect of some factor associated with day of placing, the between-days variance should not significantly exceed the within-days variance; however, it does so at even the 1 percent probability level, as can be seen by inspection of the estimated and tabulated values of F. Therefore, it may be inferred, with somewhat less than 1 chance in 100 of being wrong, that the test results were influenced by day of placing; this being true for both the 7- and 28-day compressive strengths.

*See any standard text on statistical methods.

With this knowledge, if it be desirable to reduce the over-all strength variation for the job, an initial attack would be to investigate factors associated with day of placing and provide better control over those. Numerous such factors will occur to any concrete technologist; the following are often active:

1. Uncompensated variation in moisture content of sand
2. Day to day variation in tare weight on cement scales
3. Difference in conditions under which test specimens are cured
4. Different inspectors fabricating test specimens

The degree of correlation between 7- and 28-day compressive strengths should be of interest. Using 7-day strength as the independent variable, and under the assumption that its values are not subject to error variance,* the correlation coefficient has been calculated to be $r = 0.646$, which by statistical test is significant at the 99 percent confidence level.

In Fig. C, the 7- and 28-day strengths are plotted against each other; the equation of the regression line relating the two variables is given; and the limits, of the 95 percent confidence envelope for the prediction of 28-day strength from 7-day strength, are shown. The prediction interval for these data is rather broad. For instance, if one measured the 7-day strength as 4000 psi, he could only predict—with any large degree of confidence—that the 28-day strength would lie between 4250 and 6200 psi. Whether or not this is a sufficiently accurate prediction would depend on the strength requirements of the job involved.

A third item of interest concerns Mr. Cummings' designation of a few of his extreme low values as "freaks." The context leaves some doubt as to the intended connotation of freaks. It could be inferred that the results so designated are to be ignored as being unrepresentative of the strength of the concrete, since Mr. Cummings' explanation is that they "could be due to errors in casting the cylinders, or in capping them, or even in testing them." However, any of the test results is fully as apt to have been affected by these possibilities as an extreme value. The fact that extreme values, either low or high, occur (even though rarely) is in accord with the demonstrated fact that the individual tests of the entire body of data tend to have a normal distribution. To ignore these results merely because they are the extremes of the recorded data would be a purely subjective action. The significant question is: Are these results truly freaks in the sense that they could scarcely have occurred unless some factor, which was not generally operative on this job, momentarily became so; or are they only called freakish because their occurrence, although relatively certain under the job conditions, would be infrequent? Application of extreme-value statistics gives an objective criterion for deciding this question.

*This assumption is not literally true since the 7-day strength tests are probably affected by the same factors producing "error variance" as are the 28-day strength tests. If this were evaluated and taken into account in the correlation analysis, the degree of correlation would have been indicated to be even greater.

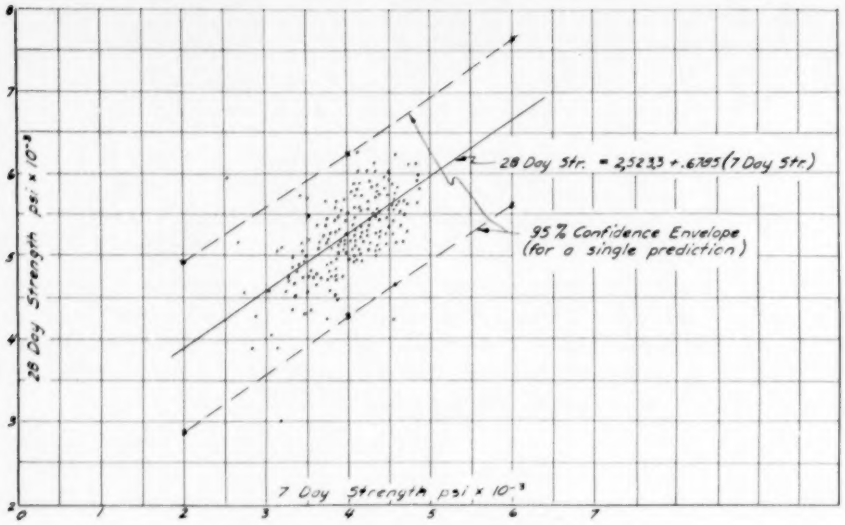
28 Day Str. = 2,523 + .679S (7 Day Str.)
95% Confidence Envelope
(for a single prediction)
28 Day Strength psi x 10⁻³
7 Day Strength psi x 10⁻³

Fig. C—7-day versus 28-day compressive strength

Just as Mr. Cummings' population of data is characterized by a normal distribution, the extreme low (or high) values in sub-sets of the data will tend to follow a different, but characteristic distribution. For our studies, one for the 7-day strengths and one for the 28-day strengths, a sub-set consisted of the strengths of cylinders fabricated during a single day's placing. These studies, conducted only on the extreme low values of the sub-sets, show for this job:

(a) A 7-day strength as low as the lowest occurring on June 1 (2430 psi) would be expected once in 9 days.

(b) A 28-day strength as low as the lowest occurring on June 4 (3003 psi) would be expected once in 63 days.

Our interpretation of this is as follows: since the 9-day "return period" for the lowest 7-day test result is well within the actual duration of the job (12 days), this result should not be classed as a freak; whereas the 63-day return period of the low 28-day test is some five times greater than the duration of the job, and hence the test could readily be so classed.

The same type of analysis can be applied to the next to the lowest (or highest) values, etc., and often produces information more useful for judging the probable success of a structure than is given by the mean and standard deviation of the entire body of tests.

Discussion of a paper by Henry J. Cowan:

Balanced Design of Prestressed Concrete Beams*

By J. E. GUEST, F. WALLEY, and AUTHOR

By J. E. GUEST†

The methods given by Prof. Cowan were previously given, not only in the papers mentioned by him, but also considerably earlier by Yves Guyon in a publication of the Institut Technique du Batiment et des Travaux Publics, Paris, in May, 1944; by P. W. Abeles in a series of articles in *Concrete and Constructional Engineering* (London) in 1948; and by the writer in a paper "Design and Construction in Prestressed Concrete," published in 1952. This paper also contained tables of the geometric properties of I-beams and T-beams covering exactly the same range of proportional sizes as those now given by Prof. Cowan.

It would be interesting to know the method to be adopted for the manufacture of the beam with pre-tensioned steel used as Example 1. Most beams with pre-tensioned steel have straight wires because of the difficulty of maintaining, during the casting and hardening of the concrete, any other profile for the wires. It is, therefore, general to keep the centroid of the wires within the kern of the section to avoid high tensile stresses at positions other than that of maximum bending moment.

While, in practice, the writer has used algebraic methods such as given by Prof. Cowan, he finds it simpler to remember that the design of a prestressed beam is based on the following:

(1) The section modulus with respect to the top or bottom of the beam is equal to the variation of bending moment divided by the permissible variation of stress on the top or bottom face.

(2) The prestressing force is equal to the product of the area of cross section and the stress due to prestressing only at the centroid of the cross section.

(3) The eccentricity of the prestressing force, that is, the position of the resultant of the forces in the wires, is at the centroid of the stress diagram for the required stresses due to prestressing only.

The variation of bending moment in (1) is the difference between the total bending moment due to loads existing after prestressing and the bending moment due to dead loads acting at the time of prestressing and which are resisted by a displacement of the prestressing force rather than an increase

*ACI JOURNAL, Apr. 1955, *Proc.* V. 51, p. 773. Disc. 51-39 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.
†Structural Engineer, Matthews & Mumby, Ltd., Manchester, England.

of its value. The variation of stress on any face is the algebraic difference between the permissible stresses on that face during the cycles of loading. For example, under a total bending moment due to an infrequently applied maximum load, the permissible stress on the bottom face may be a tensile stress of 100 psi; under dead load only the permissible stress may be 1500 psi; the variation of stress on the bottom face is, therefore, 1600 psi.

Involved algebraic expressions and notations have bedevilled prestressed concrete and given many engineers the impression that the design is much more complex than that of normal reinforced concrete; this is not so.

By F. WALLEY*

Prof. Cowan presents a table of the properties of unsymmetrical I-sections. A need of such a set of tables was realized many years ago, and was duly computed.

It may be of interest to note that tables in a far more fuller form than printed in the paper will be found as an appendix to "Prestressed Concrete Design and Construction," printed by Her Majesty's Stationery Office, London, England. This book was reviewed in the June 1954 JOURNAL ("Current Reviews," ACI JOURNAL, June 1954, *Proc.* V. 54, p. 910).

AUTHOR'S CLOSURE

The author has pleasure in acknowledging the wider scope of Mr. Walley's tables. However, the tables given in this paper were computed by the author in 1952 and the paper was submitted to ACI in 1953; the first copies of Mr. Walley's book arrived in Australia in 1954.

The author is unable to find that the method has previously been given by Guyon and Abeles, as claimed by Mr. Guest. The equations for the minimum values of the section moduli [Eq. (9) and (10)] have been included in every major treatise on the theory of prestressed concrete; while the author does not know who should be credited with having published them first, he does not, of course, claim to have done so. The equations for balanced design are, however, believed to be original. Mr. Guest does not give the full reference to his own paper, and it is presumed that he refers to a pamphlet published by the Assn. of Ship Building and Engineering Draughtsmen, London, in 1952. The author believes that this is an excellent exposition of the elementary principles of prestressed concrete design, but it does not include the theory proposed in this paper.

Mr. Guest is correct in stating that in a pre-tensioned section the wires would normally have to be straight. The maximum permissible eccentricity at the supports,† using the notation employed in the paper is:

$$e = \frac{Z_t}{A_c} + \frac{f_t Z_t}{F_o} = \frac{59}{30} + \frac{130 \times 59}{29{,}500} = 2.4 \text{ in.}$$

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†Cowan, H. J., *The Theory of Prestressed Concrete Design*, Macmillan & Co., London, awaiting publication.

This is approximately $\frac{7}{8}$ in. less than the eccentricity at midspan, and the beam should therefore be cambered by that amount. The eccentricity is thus varied while keeping the wires straight. Alternatively, a slight curvature could be given to either the top or the bottom flange if only a straight bottom or top flange is required.

The author stated, in the conclusion to his paper, that the balanced design is particularly useful for the more heavily loaded beams. In addition, the use of variable working stresses has obvious advantages for the design of partially prestressed beams. The equations derived do not replace conventional methods, but their use obviates a great deal of computation when the balanced design is economical. Mr. Guest's method could produce a balanced design only by trial and error.

The author is in entire agreement with Mr. Guest when he says that the theory of prestressed concrete design is a good deal simpler than that of normal reinforced concrete design. However, a glance at an advanced textbook will show that the algebra of normal reinforced concrete design is far from simple. It is perhaps a matter of opinion whether the designer should be encouraged to engage in a comprehensive study of the theory, or whether the algebra should be kept to the absolute minimum. The author favors the former course, since the comprehensive study of the theoretical problem saves many hours of arithmetic computation afterwards, and thus amply repays the time spent on it.

Disc. 51-40
(Covering also 51-26)

Discussion of a two-part paper by T. C. Powers and H. H. Steinour:

An Interpretation of Some Published Researches on the Alkali-Aggregate Reaction*

By GEORGE L. KALOUSEK, DUNCAN McCONNELL, ERVIN POULSEN, and AUTHORS

By GEORGE L. KALOUSEK†

Powers and Steinour in Part 1 published data on the amount of alkalies present in the liquid phase (free water) and those retained in the solids of the hydrated cement paste. The writer[1] in earlier studies on the relations between composition of gels and solutions in the system lime-soda-silica-water showed that the soda was distributed between the solution and the solids. The amounts held by the solids increased with increasing concentrations of soda in solution. Powers and Steinour now report the same kind of trend for cement pastes, but the amounts in solution relative to those retained in the solids were found to be markedly higher than those reported for the solutions in contact with synthetic gels.

Before advancing an alternate explanation to that reported by the authors for the differences in amounts of alkalies retained by the two types of solids, it may be helpful to develop a background for the discussion by briefly reviewing some of the interpretations of these data by the authors. In developing their hypothesis, Powers and Steinour assumed that the synthetic gel and cement gel differed fundamentally and advanced this as an explanation for the difference in adsorptive capacities of the two products. In further comments, they stated that the writer assumed in his earlier studies that the cement gel and synthetic gel were the same but that "later work led him to a different conclusion." The writer wishes to correct this impression as he does not recall changing his mind on the earlier assumption. Since the misunderstanding perhaps may be one of definition of terms rather than a misstatement, the following explanations are submitted. The key word may be "fundamental" used as an adjective describing the solids. The property which the writer considers to best serve as a criterion for comparing the solid phases is the structure. Therefore, to say that two solids are fundamentally the same is the same as saying the solids have the same structure. Unfortunately, this criterion of comparison is not without limitations.

*ACI JOURNAL, *Proc.* V. 51: "Part 1—The Chemical Reactions and Mechanism of Expansion," Feb. 1955, p. 497; "Part 2—A Hypothesis Concerning Safe and Unsafe Reactions with Reactive Silica in Concrete," Apr. 1955, p. 785. Disc. 51-40 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

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Structural details of these solids are not yet fully solved. Spectral data, especially that arising from x-ray diffraction, is regarded as reflecting the structure. X-ray data are, therefore, used for comparing the solids for similarities and dissimilarities. The comparisons often are not too convincing due to the weakness and diffusion of the lines of the spectrum. It may be that some day with new tools of research, it will be possible to demonstrate a fundamental difference in these solids which now are assumed to be the same or similar. Until that time comes, the scientist will have to be content with the facilities available to him notwithstanding their limitations.

The writer's views on the nature of cement gel and related synthetic gel are stated in a recent publication.[2] With the possible exception of hillebrandite forming in cement pastes cured under specified conditions, the cement gel cured at ordinary temperature appeared to be the same as the synthetic lime-rich solid obtained at room temperature as suggested by x-ray diffraction results. More recently, it has been stated by Woods and Powers[3] that afwillite is a product of hydrated cement but details of the results have not yet been seen by the writer. Bogue[4] previously stated that the hydration product of $3CaO \cdot SiO_2$ appeared to contain afwillite beside the lime-rich solid just mentioned. The possibility that still other solids may form in concrete cured with steam at a temperature of 175 F is indicated by results obtained in this laboratory. However, the solid most frequently observed manifested the same x-ray pattern as the lime-rich hydrate isolated in the system lime-silica-water at room temperature.

Although the products are structurally the same or similar, the physical properties and chemical composition may vary markedly. One apparent reason for this difference[2] may be the accommodation of the Al_2O_3, and possibly also the Fe_2O_3, in the lattice of the hydrous calcium silicate as this phase forms in hydrating cement. It is true that even though the cell size of the product with addition of Al_2O_3 may change, the structure remains unaltered.

A plausible explanation for the difference in adsorptive capacities of the cement and synthetic gel for alkalies is as follows. If the hypothesis as advanced by Powers and Steinour is correct that alkalies are adsorbed by either of the two types of gel, the amount should be dependent largely on the amount of surface available for adsorption. The surface areas available to nitrogen gas should be a measure of such surfaces. The apparent surface areas measured by water adsorption cannot be used because a part of the water apparently is accommodated differently from the nitrogen, probably being adsorbed in the crystal lattice. The gels studied by the writer in the ternary system, and ones highly similar to those containing relatively small amounts of soda, manifested surface areas available to nitrogen of about 100 to 180 sq m per g. Hydrated cement as demonstrated by Blaine and Valis[5] has a surface area available to nitrogen of about 20 to 30 sq m per g. Since the surface of the synthetic gel is roughly 5 to 10 times as large as that of the cement gel, it would follow that the synthetic product would adsorb markedly more soda than does the cement gel. It follows, therefore, that this difference

in surface area alone could account for the difference in adsorptive property between the cement and synthetic gels. It is not surprising that the synthetic gels have a greater surface area since they result from a condition that favors a more rapid formation than that which occurs when the constituents must leach from and diffuse through a cement paste.

The data as reported and applied by the authors indicate why a low alkali content in a cement may not alone be an assurance that the alkali-aggregate reaction will not occur. If warm, humid weather occurs for a time while a cement hydrates in a concrete containing reactive aggregate and is then followed by dry weather the concentrations of alkali that dissolved would, of course, increase as the amount of free water evaporates. The alkali concentration could attain dangerous levels even for cements of relatively low alkali content. For this reason, changes in weather conditions may be an important factor affecting the alkali-aggregate reaction for cements of lower alkali contents, perhaps those containing as little as 0.25 percent alkali.

Although the hypothesis of Powers and Steinour explains some aspects of the alkali-aggregate reaction, it appears to be of little help in accounting for some significant factors involved in the phenomenon. For example, the interesting inhibition of the reaction by additions of lithium hydroxide[6] (or salts which sooner or later convert to the hydroxide) is not explained by the hypothesis. Could it be, in considering the apparent differences in behavior of the three alkalies involved that the size of the Li^+, Na^+, and K^+ ions, 0.60, 0.95, and 1.33 A radii, respectively, has an effect? Since the phenomenon of alkali-aggregate reaction is in part a transfer of ions through a membrane, then one might speculate how the mechanism would differ between the lithium and the two common alkalies. The Li^+ ion could diffuse through the membrane with greater ease than the other two ions. This and other speculative ideas in addition to those reported by Powers and Steinour could be advanced in an attempt to explain the mechanism of the alkali-aggregate reaction. It appears, as pointed out by the authors, that more experimental data are required to develop a theory which will afford an explanation for the role of all the variables affecting the alkali-aggregate reaction.

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By DUNCAN McCONNELL*

The elaborate statement of the mechanism of alkali-aggregate reaction by the authors is a worthy undertaking to the extent that it resolves certain supposed points of divergence in several published interpretations. Certainly this presentation contains numerous details that were merely alluded to in the paper by McConnell, Mielenz, Holland, and Greene.[1] Nevertheless, I see no fundamental differences between the statements made then[1] and the more complete presentation under discussion.

For example, we[1] stated: "As a consequence of the formation of gels and their inhibition of water, osmotic pressures accumulate, producing stresses which distend and may ultimately rupture the paste immediately surrounding the reactive particle." This statement does not indicate whether we regarded the pressure as a hydraulic pressure or a swelling pressure because, like the present authors, we found no necessity for making a sharp distinction between these phenomena. "During their early history, the gels are probably highly viscous, but their viscosity is decreased as water is imbibed from the paste," can surely be interpreted to mean that we believed that these materials could represent all stages of viscosity, from something quite rigid to something fairly fluid. The term "solid" was deliberately avoided inasmuch as this might imply crystalline organization of the substance. Even though such crystalline organization might have existed, it would be destroyed after alteration had begun, and opal and natural glasses lack such organization in their original condition.

However, in considering the swelling of a crystalline solid, such as a montmorillonite-type clay, as the authors have done (p. 508), it seems to me that certain differences preclude a straightforward comparison. For the clay it seems that the crystallinity is essentially preserved, and Forslind[2] and others have postulated a structural arrangement of the water within these highly expanded crystals. That hydroxyl ions may enter into organized structures, even in such a manner as to replace silicon atoms, has been suggested.[3] On the basis of this premise I have attempted to explain some of the unusual properties of montmorillonite.[4]

I cannot concur that the expansion of such a solid is strictly comparable to that of a gel or noncrystalline material. In my opinion the thermodynamic stabilities should be different. Water enters the montmorillonite in such a manner that arrangement presumably exists between the silicate sheets of the clay mineral and the water layers which contribute to the enlargement of this crystal, whereas no organized arrangement can be assumed to exist between the ultimate particles of the gel and the liquid phase that is adsorbed by this system. As a consequence of the greater stability of the crystalline system, a closer approach to true reversibility during many cycles of wetting and drying would be expected.

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The question of the regeneration of alkali through the interaction between calcium hydroxide and previously formed alkali-silica gel is not new, as the authors fully realize. Prior to 1947 we[1] examined many thin sections of concrete to discover evidences concerning the quantity, distribution, and crystalline size of calcium hydroxide that might have some bearing on regeneration. Our results are summarized (pp. 104-105)[1] up to 1947. No essential differences were discovered between the modes of occurrence of calcium hydroxide in "good" and "bad" concretes, with the possible exception of concretes which had been subjected to frost action, so that these findings neither confirm nor deny the hypothesis of the authors.

More important, however, is the fact that the authors have not shown any analytical data on gels that would tend to indicate that a material which presumably formed as an alkali-silica gel later contains a significant amount of *CaO*. For the gels analyzed by the U. S. Bureau of Reclamation chemists (Table 1)[1] nothing remotely resembling the *Ca:Si* ratio suggested by the authors is evident. In addition, such gels apparently are capable of reaching the surface as exudations without interacting with calcium hydroxide to any significant extent.

Kalousek[5,6] has suggested the formation of a peripheral zone of alkali-calcium-silicate gel about the reactive aggregate grain, but he does not imply any such diffusion, mixing, and interaction of constituents as do the authors. That the extent of diffusion and interaction which the authors suppose is largely imaginary is implied by the fact that even old concretes appear to contain unhydrated clinker grains.

Although the authors have presented an elaborate theoretical statement, based largely on the attainment of chemical equilibria in concretes undergoing alkali-aggregate reaction, they have not been able to demonstrate that such conditions exist. Thus their hypothesis, in itself, appears to be unsuccessful in eliminating several alternative interpretations.

In the absence of any straightforward evidence to the contrary, one must surely agree with the authors' statement: "Expansion is produced when the alkali-silica complex imbibes water." Beyond this point, there must be many ways in which the extremely complex details can be explained.

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By ERVIN POULSEN*

There is no doubt that the hypothesis concerning safe and unsafe reactions with cement and reactive silica in concrete as published by Messrs. Powers and Steinour puts the problems about cement-aggregate reactions in a new perspective.

The hypothesis has proved to be an excellent working hypothesis for the Danish Committee on Alkali-Silica Reactions in Concrete. This discussion represents a brief summary of the results obtained from the treatment of the hypothesis.

REACTIONS WITH SPHERICAL SINGLE-SIZE REACTIVE SILICA AND HIGH-ALKALI CEMENT

The following transcription of basic Eq. (5) is used:

$$\frac{x}{w_e} = \frac{\dfrac{Na_2O}{c}100}{\dfrac{w_e}{c}100 + 15mk\left(\dfrac{A}{c}\right)\left(\dfrac{A_r}{A}100\right)} \qquad \text{(a)}$$

where A is the total amount of aggregates and kA_r is the total reactive silica in the system (equal to the notation SiO_2). The factor k is the fraction of the reactive part of the aggregate, which is 100 percent reactive. The range of k is $0 < k \leqq 1$.

By using the following data:

1. $w/c = 0.50$ by weight (often used in mortar bar tests)

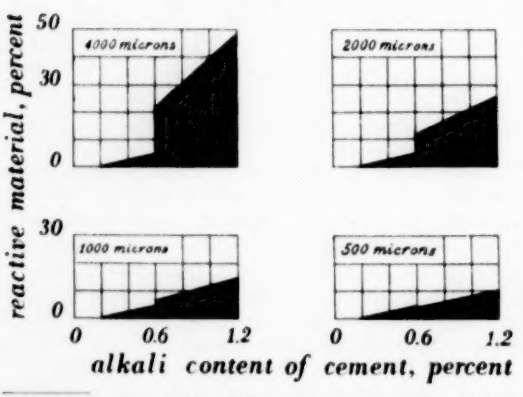

Fig. A—The unsafe area in relation to the diameter of the reactive single-size silica

The area increases greatly by increasing the diameter. As a safe surface layer of 150 microns is assumed, all mixes with reactive material consisting of grains all of less than 300 microns will be safe. The figure also shows that the area found in the range from 0 to 0.6 percentage of alkali in cement does not seem to be correct. Compare the shape of the unsafe area with Fig. C and D.

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2. $m = 1 - \left(1 - 2\frac{\delta_0}{d}\right)^3$, where δ_0 is the safe surface layer and d is the diameter of the single-size and spherical reactive aggregate
3. $x/w_e = 4 \times 10^{-3}$ by weight, which is assumed from the studies of safe reactions with high-alkali cement by means of the basic equation
4. $A/c = 2.0$ by weight (often used in mortar bar tests)

we get the following equation:

$$k\left(\frac{A_r}{A}100\right) = \frac{8.35}{1 - \left(1 - 2\frac{\delta_0}{d}\right)^3}\left(\frac{Na_2O}{c}100\right) - \frac{1.17}{1 - \left(1 - 2\frac{\delta_0}{d}\right)^3} \quad \text{.....(b)}$$

which is known to be a straight line when $(A_r/A)100$ and $(Na_2O/c)100$ are used as coordinates and d is kept constant. Eq. (b) determines the borderline between safe and unsafe reactions.

REACTIONS WITH REACTIVE SILICA AND LOW-ALKALI CEMENT

In Eq. (a) the following data are used:

1. $w/c = 0.50$ by weight
2. $m = 1$ (all material has reacted)
3. $x/w_e = 2.5 \times 10^{-3}$ by weight, as found by Powers and Steinour
4. $A/c = 2.0$ by weight

We get the following equation:

$$k\left(\frac{A_r}{A}100\right) = 13.3\left(\frac{Na_2O}{c}100\right) - 1.17 \quad \text{.....(c)}$$

which is known to be a straight line when $(A_r/A)100$ and $(Na_2O/c)100$ are used as coordinates. Eq. (c) should give the borderline between safe and unsafe reactions.

It is seen that the line is independent of the diameter d. Experience shows that the diameter of reactive particles has some influence, but it seems impossible from the criteria $m = 1$ and $x/w_e = 2.5$ g per l alone to find an analytical relationship for the influence of the diameter of the reactive material and further studies have to be made.

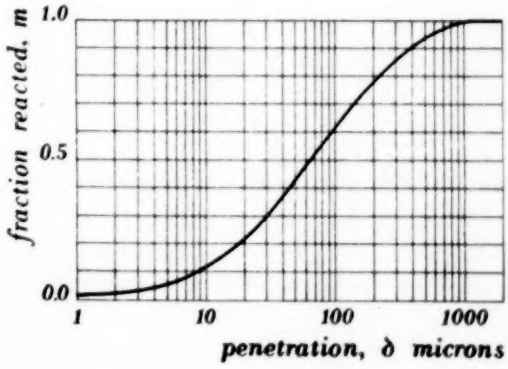


Fig. B—The fraction reaction, m, for Danish standard gradation used in mortar bar tests in relation to the penetration δ

SAFE AND UNSAFE MIXES WITH SPHERICAL SINGLE-SIZE REACTIVE SILICA

If the two lines from Eq. (b) and (c) are drawn, we get a picture as shown in Fig. A. The figure shows the shape of the "unsafe area" for different values of diameter d. To draw the lines, δ_0 and k were assumed to be 150 microns and 1, respectively.

REACTIONS WITH SPHERICAL GRADED REACTIVE SILICA AND HIGH-AKLALI CEMENT

When the diameter of the aggregates in a mix have several sizes, Eq. (a) may still be used. To obtain the correct value of m it is presupposed that the safe surface layer is the same for all the aggregate.* The reactive silica is divided into v groups. The group named i has n_i grains, all with diameters of d_i. According to this, m may be written

$$m = \frac{\Sigma\, n_i\,(d_i^3 - (d_i - 2\delta_0)^3)}{\Sigma\, n_i d_i^3}$$

or

$$m = 6\delta_0 \frac{\Sigma\, n_i d_i^2}{\Sigma\, n_i d_i^3} - 12\delta_0^2 \frac{\Sigma\, n_i d_i}{\Sigma\, n_i d_i^3} + 8\delta_0^3 \frac{\Sigma n_i}{\Sigma n_i d_i^3}$$

If group i has the weight p_i, and the same unit weight is assumed, m could be written in the following way, where P is the total weight of the reactive silica.

$$m = \frac{6\delta_0}{P} \sum \frac{p_i}{d_i} - \frac{12\delta_0^2}{P} \sum \frac{p_i}{d_i^2} + \frac{8\delta_0^3}{P} \sum \frac{p_i}{d_i^3} \qquad \text{(d)}$$

The distribution p_i may be found by test and Eq. (d) should be used in Eq. (b) instead of $m = 1 - [1 - 2\,(\delta_0/d)]^3$.

It must be remembered that the minimum d_i must be larger than $2\delta_0$. If that is not the case, m should be calculated only by taking d_i in account as long as $d_i < 2\,\delta_0$ because the grains with diameter d_i have finished their reaction when a penetration of $\frac{1}{2}\,d_i$ is reached.

This may be written

$$m = \frac{\sum_{d=d_0}^{d=2\delta_0} n_i d_i^3 + \sum_{d=2\delta_0}^{d=D_0} n_i[d_i^3 - (d_i - 2\delta_0)^3]}{\sum_{d=d_0}^{d=D_0} n_i d_i^3}$$

where d_0 and D_0 are the minimum and maximum diameter respectively of the whole group of reactive grains.

If group i has the weight p_i, and the same unit weight is assumed, m may be written in the following way, where P is the total weight of the reactive silica.

*The presupposition is not always valid and the computation can be made for varying thickness of the safe surface layer, but the equation will, of course, be more complicated.

$$m = \frac{1}{P}\sum_{d=d_0}^{d=2\delta_0} p_i + \frac{6\delta_0}{P}\sum_{d=2\delta_0}^{d=D_0}\frac{p_i}{d_i} - \frac{12\delta_0^2}{P}\sum_{d=2\delta_0}^{d=D_0}\frac{p_i}{d_i^2} + \frac{8\delta_0^3}{P}\sum_{d=2\delta_0}^{d=D_0}\frac{p_i}{d_i^3} \quad \text{(e)}$$

When δ_0 is very small compared to the diameter, m may be written

$$m = \frac{6\delta_0}{P}\sum\frac{P_i}{d_i} = 6\,\frac{\delta_0}{d_m} \quad \text{(f)}$$

where d_m represents an average diameter. This equation, however, may be used for stone only ($d \geqq \frac{1}{2}$ in.).

Example 1

If the distribution p_i between two numbers of sieves is assumed to be constant *e.g.*, $p_i = P/v$, we get

$$d_m = \frac{v}{\sum\frac{1}{d_i}}$$

If the subintervals are chosen all equal to Δd in size and if the maximum and minimum diameters of the grains are D_0 and d_0, respectively, we get

$$v = \frac{D_0 - d_0}{\Delta d}$$

and

$$d_m = \lim_{\Delta d \to 0}\frac{D_0 - d_0}{\frac{\Delta d}{d}} = \frac{D_0 - d_0}{\int_{d_0}^{D_0}\frac{dd}{d}} = \frac{D_0 - d_0}{\ln\frac{D_0}{d_0}}$$

If $D_0 = 2\,d_0$ (as is the case in Danish standard sieves) we get $d_m = 0.72\,D_0$. If, for instance, a concrete specimen is cast with reactive aggregate $\frac{1}{2}$-1 in. in size and high-alkali cement, Eq. (b) may be used with d equal to 0.72 in. $\approx \frac{3}{4}$ in. If another distribution p_i is assumed, another value of d_m is found, but only in special cases will d_m differ much from the value found here.

Example 2

The graduation p_i of the reactive aggregate used in Danish standard mortar bar tests is as follows:

diameter, mm	d_i	p_i, g	$\frac{p_i}{d_i}$	$\frac{p_i}{d_i^2}$	$\frac{p_i}{d_i^3}$
4–2	3	20	20/3	20/9	20/27
2–1	3/2	20	40/3	80/9	160/27
1–½	3/4	20	80/3	320/9	1280/27
½–¼	3/8	20	160/3	1280/9	10,240/27
¼–⅛	3/16	20	320/3	5120/9	81,920/27
Sum		100	620/3	6820/9	93,620/27

If $\delta_0 \leqq 1/16$ mm, m may be written, according to Eq. (d), as follows:

$$m = \frac{6\delta_0}{100} \times \frac{620}{3} - \frac{12\delta_0^2}{100} \times \frac{6820}{9} + \frac{8\delta_0^3}{100} \times \frac{93{,}620}{27} = \delta_0 (12.4 - 91\delta_0 + 277\delta_0^2)$$

When $d_0 > 1/16$ mm, m should be calculated by means of Eq. (e). The results of the calculation are given in Fig. B, where values for $\delta_0 \leqq 1/16$ mm also are given.

The basic equation for this standard aggregate may then be written

$$\left(\frac{A_r}{A} 100\right) km = 8.35 \left(\frac{Na_2O}{c} 100\right) - 1.17 \quad \text{(g)}$$

where m is taken from Fig. B.

If the safe surface layer δ_0 is assumed to be 125 microns, we get $m = 0.67$. For $k = 0.8$ Eq. (g) will be

$$\left(\frac{A_r}{A} 100\right) = 15.7 \left(\frac{Na_2O}{c} 100\right) - 2.2$$

If the alkali content is $Na_2O/c)100 = 1$ percent, expansion will only occur when the reactive part of the total amount of the aggregate is less than 13-14 percent. In case we have (Na_2O/c) $100 = 0.6$ percent, expansion will only occur when the reactive part of the total amount of the aggregate is less than about 7 percent.

TABLE A—EXPANSION OF MORTAR BARS AT 18 MONTHS*

Cement No.	$\frac{Na_2O}{c}$ 100, percent	Percent opal in sand		
		1	2	5
1	0.22	0.18	0.03	0.00
2	0.23	0.29	0.08	0.04
3	0.24	0.11	0.02	0.00
4	0.27	0.24	0.03	0.00
5	0.29	0.23	0.03	0.01
6	0.29	*0.25*	0.04	−0.01
7	0.36	0.50	0.18	0.00
8	0.39	—	*0.35*	0.03
9	0.43	*0.40*	*0.25*	0.02
10	0.51	0.49	0.93	0.03
11	0.53	*0.40*	*0.35*	0.03
12	0.54	*0.55*	*0.50*	0.04
13	0.54	0.44	0.61	0.12
14	0.58	0.56	0.69	0.10
15	0.59	*0.65*	—	0.10
16	0.60	0.43	0.60	0.08
17	0.61	—	—	—
18	0.62	*0.55*	*0.75*	0.09
19	0.63	0.44	0.56	0.08
20	0.63	—	—	—
21	0.65	—	—	—
22	0.67	*0.55*	—	*0.25*
23	0.67	0.42	0.64	0.06
24	0.68	—	—	—
25	0.69	0.61	0.61	0.06
26	0.71	—	—	—
27	0.72	0.37	0.67	0.12
28	0.74	—	—	—
29	0.77	0.40	0.75	0.20
30	0.80	0.52	0.90	0.48
31	0.80	0.46	0.83	0.33
32	0.82	0.20	0.46	0.15
33	0.84	—	—	—
34	0.93	—	—	—
35	0.99	0.41	0.81	0.78
36	0.99	—	—	—

*Data published by D. O. Woolf.
Note: Results in italics are extrapolated from the original test data. Missing results are due to difficulties in extrapolating with sufficient accuracy.

If the reactive material had been all 2-4 mm in diameter, m would have been about 0.25 if $\delta_0 = 125$ microns was assumed. With k equal to 0.8 we get

$$\left(\frac{A_r}{A} 100\right) = 42 \left(\frac{Na_2O}{c} 100\right) - 6$$

which shows that if $(Na_2O/c)100 = 1$ percent, expansion will occur when reactive material is less than about 36 percent. In case of $(Na_2O/c)100 = 0.6$ percent, expansion will occur when the reactive material is less than about 19 percent. It is seen, which is already known, that single-size aggregate is deleteriously reactive within a larger range of added amount of reactive material than graded aggregate.

DATA PUBLISHED BY D. O. WOOLF

In 1952 a paper by D. O. Woolf* gave a great number of test results. These are—as far as known—the only test results published which, for a given material, have sufficient data for an investigation of the range of the alkali content of cement and reactive material. However, the results of the investigations may show some deviations because 36 types of cement were used, and it has been necessary in some cases in this study to extrapolate to get 18 months' results (Table A).

Re-examination of data†

The notations used are as follows:

x = alkali content of the cement in hundredths of a percent. This x has nothing to do with the x in Eq. (a)

y = reactive material in percent

z = expansion of the mortar bars in hundredths of a percent

A regression analysis was made on the basis of the following expression:

$$z = \alpha x^{\beta} y^{\gamma} e^{\delta x/y}$$

where α, β, γ, and δ are constant factors and e is the base of natural logarithms. By taking the logarithms on both sides we have a

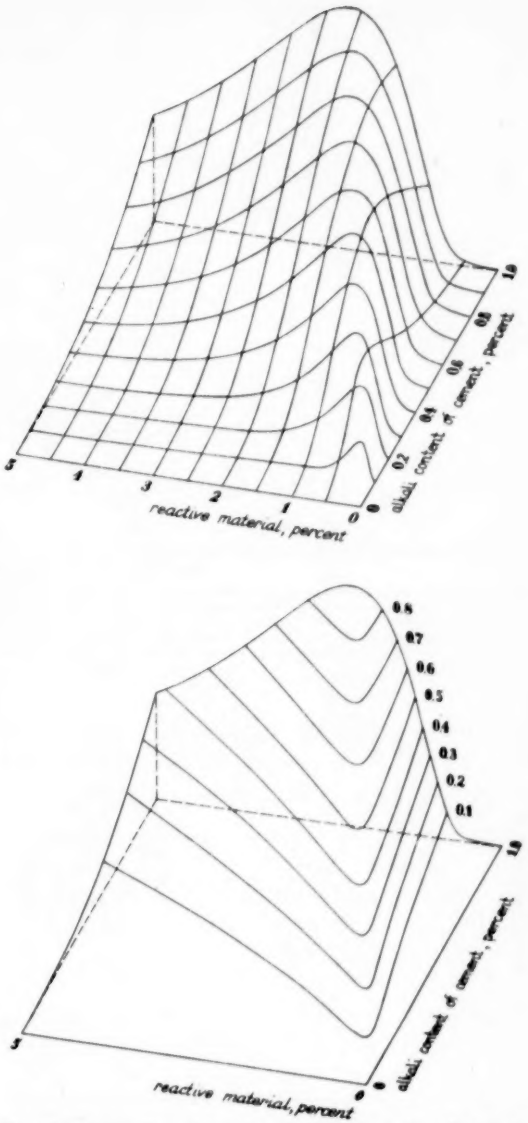


Fig. C—Perspective view of the expansions in relation to reactive material and alkali content of cement (Data from D. O. Woolf)

(Top) Cross sections for constant reactive material and constant alkali content in cement. The pessimum proportion varies with the alkali content of the cement used. Notice that the slope is steep in the area between the pessimum proportion and the alkali axis, i.e., a given change of the opal content of the sand is very dangerous in the area mentioned. (Bottom) Levels of equal expansion (the same as in Fig. D).

*"Reaction of Aggregate with Low-Alkali Cement," *Public Roads*, Aug. 1952, p. 50.

†The author acknowledges the assistance rendered by Per Bredsdorff and A. Nørgård in dealing with the statistical analysis in the re-examination of Woolf's data.

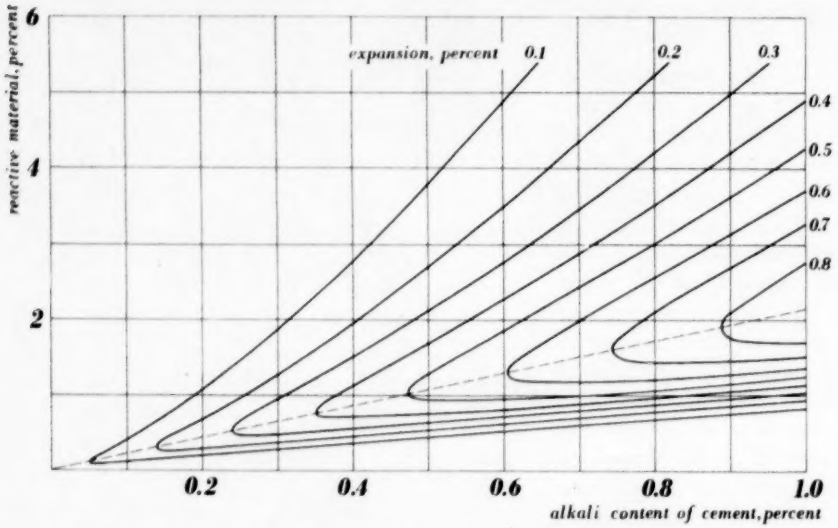


Fig. D—Expansion of mortar bars in relation to reactive material and alkali content of cement (Data from D. C. Woolf)

Each solid curve shows combinations of reactive material and alkali which give equal expansion. Dotted line determines the pessimum proportions. Compare the shape of the unsafe area with the shape found by the hypothesis, Fig. A. Size of opal particles used by Woolf is between 590-1190 microns. Eq. (h) does not give the curve for "no expansion." However, a line for no expansion was assumed from the data by Woolf. A calculation showed that from Eq. (b) the safe surface layer, δ_0, should be about 85 microns. Comparison with the results mentioned by Powers and Steinour seems to be fairly good.

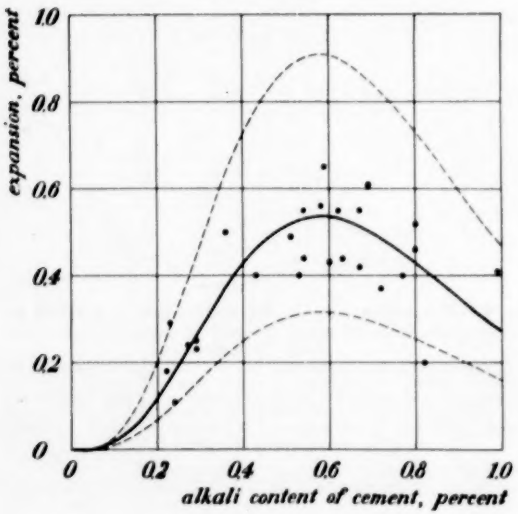


Fig. E—Expansion of mortar bars at 18 months. Aggregates used is 1 percent single-size opal in sand (Data from D. O. Woolf)

Solid curve shows the cross section in Fig. C (top) for reactive material equal to 1 percent. Dotted curves shown are plus and minus one standard deviation, respectively.

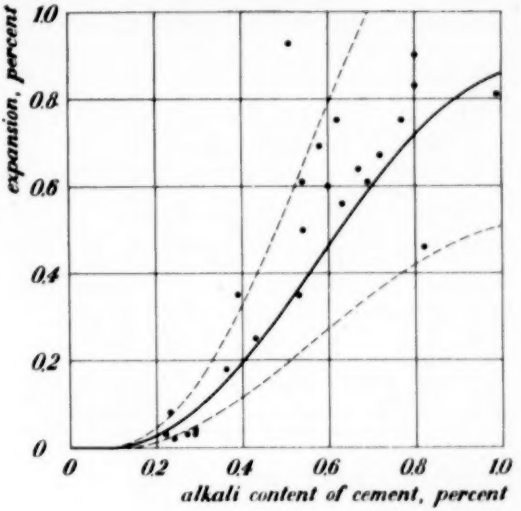


Fig. F—Expansion of mortar bars at 18 months. Aggregate used is 2 percent of single-size opal in sand (Data from D. O. Woolf)

Solid curve shows the cross section in Fig. C (top) for reactive material equal to 2 percent. Dotted curves shown are the expression, Eq. (h), plus and minus one standard deviation, respectively.

linear regression analysis. By means of the method of least squares the following expression was obtained:

$$z = \frac{x^{3.76}}{1837y^{3.01}} \times 10^{-0.0282\,x/y} \qquad \text{(h)}$$

where the parameters are significantly different from zero. The standard deviation found was $s = 0.229$.

It is seen that $z = 0$ for $x = 0$ and $y \neq 0$, and $z \to 0$ for $y \to 0$. It is a disadvantage that it is impossible to draw a satisfactory level for "no expansion" (see Fig. C and D). The level for "no expansion" may be defined

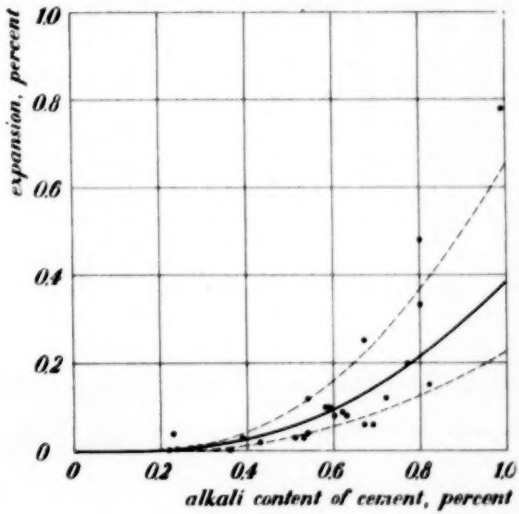
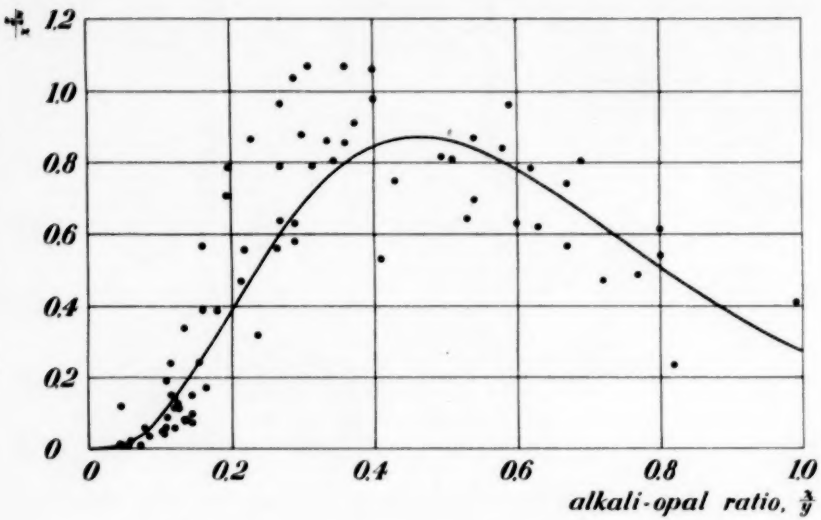


Fig. G—Expansion of mortar bars at 18 months. Aggregate used is 5 percent of single-size opal in sand (Data from D. O. Woolf)

Solid curve shows the cross section in Fig. C (top) for reactive material equal to 5 percent. Dotted curves shown are the expression, Eq. (h), plus and minus and one standard deviation, respectively.



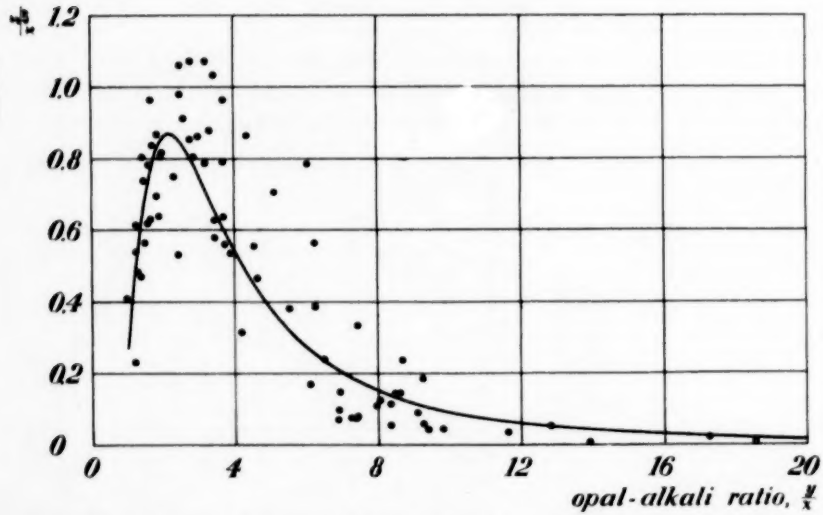


Fig. H—Relation between $z/x^{0.75}$ and x/y (top) and y/x (bottom), respectively (Data from D. O. Woolf)

The graphs show a transcripiton of Fig. C into two dimensions; x, y, and z are alkali content of cement, reactive material in sand, and expansion, respectively.

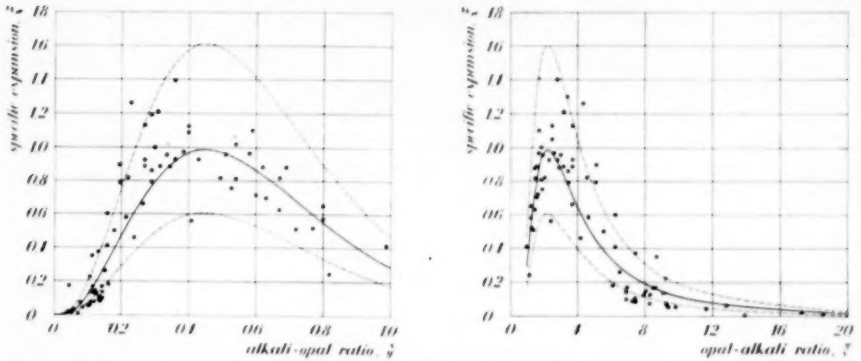


Fig. 1—Relation between specific expansion z/x and x/y (left) and y/x (right), respectively (Data from D. O. Woolf)

Terms x, y, and z are alkali content of cement, reactive material in sand, and expansion, respectively. Dotted curves are the expression found, plus and minus one standard deviation.

as $z = 0.04$ percent, but it does not work out in the right way. The level for no expansion should be used for finding the constants of the reactive materials.

A perspective view of the graphical outlook is given in Fig. C. In Fig. E, F, and G the cross sections for $y = 1$, 2, and 5 percent, respectively, are given. It is seen that a better result may be found. This could probably be done if the regression analysis was changed a little, so that small values of z were not given the same weight as the greater values.

The expression found, Eq. (h), is not handy when a graph is drawn. Therefore the equation is changed so that we get

$$\frac{z}{x^{0.75}} = \frac{1}{1837}\left(\frac{x}{y}\right)^{3.01} \times 10^{-0.0283\,x/y}$$

This may be drawn easily when $z/x^{0.75}$ and x/y are used as coordinates (see Fig. H). However, x may be used instead of $x^{0.75}$ (see Fig. I). This may give a physical explanation and the following expression was tried

$$\frac{z}{x} = \frac{1}{1837}\left(\frac{x}{y}\right)^{\alpha} \times 10^{-0.0283\,y/x}$$

The result of the test was that it is possible to say that the *specific expansion* z_s (expansion per unit of alkali) *depends on the specific alkali content* x_s (alkali content per unit reactive material) in the following way:

$$z_s = \alpha x_s^{\beta}\, 10^{\gamma x_s}$$

The equation found was:

$$z_s = \frac{1}{1837}\, x_s^{2.94} \times 10^{-0.0282 x_s}$$

The standard deviation found is $s = 0.212$.

The graph used by D. O. Woolf (Fig. G) was tried also (see Fig. J); the equation found was:

$$z = \frac{1}{202}\left(\frac{x}{y}\right)^{3.25} \times 10^{-0.0284\,x/y}$$

The standard deviation found is $s = 0.257$.

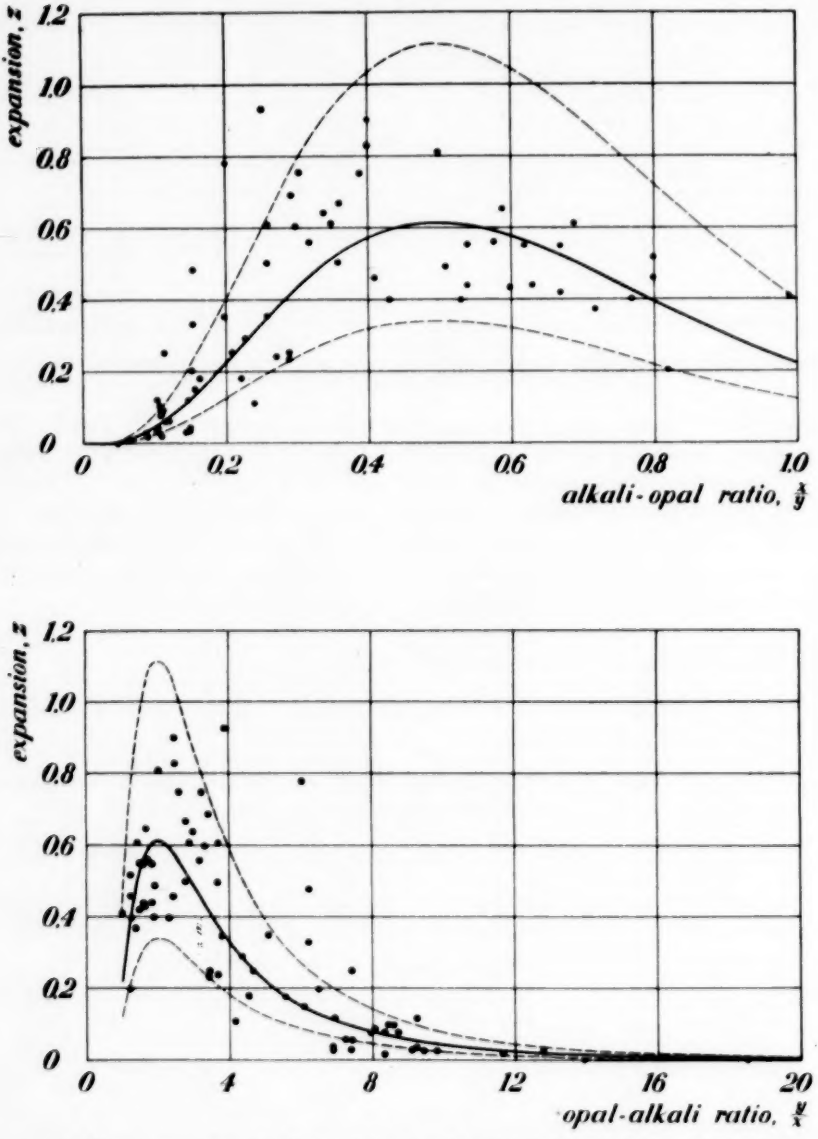


Fig. J—Relation between z and alkali-opal ratio x/y (top) and opal-alkali ratio y/x (bottom), respectively (Data from D. O. Woolf)

Terms x, y, and z are alkali content of cement, reactive material in sand, and expansion, respectively. Dotted curves are the expression found plus and minus one standard deviation.

FUTURE RESEARCH

To study the hypothesis given, the Danish Committee on Alkali-Silica Reactions in Concrete has started an investigation of the expansion of mortar bars. The alkali contents vary in the range 0.35-1.15 percent, and the range of the reactive material is from 0 to about 64 percent of total aggregate. Five different sizes of grains and five different materials from dense to porous chert are being used.

AUTHORS' CLOSURE

We are gratified to find that Professor McConnell concurs with most of the views expressed in Part 1. From a reading of the paper by McConnell, Mielenz, Holland, and Greene, we gained the impression that Professor McConnell and his colleagues thought of osmotic pressure in terms of hydraulic pressure. This seemed to be indicated by the experiments that they described, demonstrating the development of hydraulic pressure by osmosis. The use of the word "viscous" in the sentence quoted by Professor McConnell rather strengthened that impression for we associated that term with hydraulic phenomena. However, it now appears that the word was used to suggest a degree of solidity that would permit development of destructive swelling pressure without total confinement of the swelling medium.

Conversation with various persons indicates that not everyone understands as does Professor McConnell, why it is unnecessary to make a sharp distinction between mechanisms producing swelling pressure and hydraulic pressure. We hope, therefore, that our explanation has served some useful purpose.

It was not our purpose to imply that there is a close analogy between the mechanism of swelling produced by alkali-aggregate reaction and that produced by the entry of water into certain crystals. We meant to cite this only as one example of a system that could produce swelling pressure.

Relative to gel composition, we would expect what Professor McConnell reports in his comments on Part 2. We believe that the lime reaction produces (in concrete) a relatively solid gel. We would expect to find it primarily only in reaction rims, or in more extensive in situ alterations of aggregate grains. Of the three natural gels in the table that Professor McConnell cites, two are exudations, and the other is presumably a similar relatively fluid gel. We would expect these to be alkali-silica gels of low CaO content, as was found, for such gels can imbibe enough water to become fluid. When the viscous gel breaks through to the surface, it presumably leaves a thin layer of lime-alkali-silica complex at the boundaries marking its passage; but the gel that escapes should, we think, contain little lime.

Referring, apparently, to our supposition that opal particles can react completely with alkali or, in the case of safe reactions, with lime and alkali, Professor McConnell suggests that ". . . the extent of diffusion and interaction which the authors suppose is largely imaginary . . . " He says this is "implied by the fact that even old concretes appear to contain unhydrated

clinker grains." The fact that unhydrated cores of the original grains can be found in old concretes seems to us rather too loose an analogy from which to reason regarding rates when a considerably different reaction is involved. We think, as a matter of fact, that the normal cement reaction proceeds remarkably well in set and largely solidified concrete. Some might reason that a reaction could not long continue under such conditions, but it can.

A paper by Gaskin, Jones, and Vivian (*Australian Journal of Applied Science*, V. 6, No. 1, Mar. 1955, pp. 78-87) has just come to hand that seems pertinent. It tells of experiments in which opal particles were embedded in cement paste containing radioactive sodium. Autoradiographs showed with opal particles ". . . very considerable accumulation (of alkali) throughout the particles after storage at room temperature for only one week." (Other forms of alkali generally showed only surface attack.) Thus, at least with respect to the alkali reaction, it seems that our supposition is justified.

Professor McConnell considers us unsuccessful in two tasks that we did not attempt. We have not yet attempted to test the hypothesis except through appeal to already known facts. One reason for publishing at this stage was the hope that we might interest others also in the perhaps difficult laboratory studies that (as we said) will be required for an adequate test of the hypothesis.

Likewise, we have been "unsuccessful in eliminating several alternative explanations," because that was not our object. We did not find in the literature any detailed explanation that covered all the ground we wished to cover. We found only partial explanations, and we attempted a more complete one. It is perhaps an "elaborate theoretical statement" but then, as Professor McConnell says, there are undoubtedly "extremely complex details" in the actual situation. There may be "many ways" in which they can be explained, but we do not see how they are ever to be explained correctly unless somebody is willing to use imagination.

We said (p. 502): "Kalousek assumed that the calcium silicate product formed in hydrated portland cement might be fundamentally like the gels formed in his experiments"; and we inserted the footnote: "Later work led him to a different conclusion." In writing the footnote, we had in mind the work of Kalousek and Adams (ACI JOURNAL, Sept. 1951, *Proc.* V. 48, pp. 77-90). In that paper it was concluded that "phase X" is formed in portland cement paste, and that this phase "may be a gel consisting of all oxide constituents of cement." Since the chemical constitution was thus regarded as being different from that of synthetic calcium silicates, it seemed to us that Dr. Kalousek regarded them as fundamentally different materials. Dr. Kalousek's explanation now clarifies the matter: although the products are chemically different, they are considered to be structurally alike.

Dr. Kalousek says that our theory fails to explain the effects of inhibitors. It is true that we did not deal specifically with this question. However, we believe that the effects of inhibitors can be explained in terms of our hypothesis. George Verbeck of the Portland Cement Assn. Laboratories has de-

veloped a hypothesis centered around relative rates of diffusion of various ions. Since, by our hypothesis, "we are dealing with a complex problem in diffusion leading under some conditions to an expansive reaction and under others to a safe reaction" (p. 794), it follows that anything that will alter relative rates of diffusion of the ions involved will alter the conditions producing a just-safe reaction. An inhibitor may be a substance that alters relative diffusion rates in a beneficial way.

Dr. Kalousek refers to remarks by Woods and Powers to the effect that afwillite is a product of cement hydration. The remarks referred to were either incorrect or misunderstood, for we have no experimental evidence that hydrated cement contains afwillite. All we know is that when pure tricalcium silicate is ball-milled with water, the hydration product is afwillite, and that the hydration product of C_3S when hydrated as a paste is not afwillite.

We find it highly gratifying that the Danish Committee on Alkali-Silica Reactions in Concrete is undertaking laboratory studies based on our hypothesis. Particularly, we appreciate the analysis presented by Professor Poulsen. We must point out, however, that Professor Poulsen's Eq. (c) embodies an assumption that is not compatible with our hypothesis. The assumption is that the terminal alkali concentration for low-alkali cements can be treated as a constant. As pointed out in the next to last paragraph on p. 801, the final alkali concentration in a just-safe reaction should be lower the larger the particle size of the reactive silica.

We wrote Professor Poulsen about this and learned that we had gained somewhat the wrong impression as to the nature and purpose of his discussion. In a letter to us, dated Oct. 17, 1955, he said:

"I know that the presuppositions for the analytical relationship are not fully pointed out. Besides this I have not stated my way of analyzing and I am glad to get this opportunity to do so.

"When a problem has to be solved 'from the writing desk,' as we say in Denmark, it is necessary to realize three things, *viz.*,: the facts, the presuppositions, and the wanted.

"When a problem is split up into the above mentioned items, 'the wanted' (the results) could be found just by mathematical manipulations. Four things may happen to the results.

1. The results differ much from the experimental knowledge.
2. The results differ slightly from experimental knowledge.
3. The results are exactly in accordance with the experimental knowledge.
4. The results are not confirmed by experimental data over the whole range.

"In the first case only, the presuppositions are wrong and new presuppositions have to be made.

"In the second case, the presuppositions are useful.

"In the third case, the presuppositions may not be right but they are useful.

"In the fourth case, experiments have to prove or disprove the results.

"The above described way is the way I have worked with the problems."

From this and other passages in the letter, it became apparent that Professor

Poulsen was simply testing the consequences of adopting certain simplifying assumptions. At the time of writing this discussion, he saw no way to avoid the assumption that we have just questioned, but later, as indicated in his letter, he found that he could treat the terminal value of alkali concentration as a function of particle diameter, and arrived at the following instead of his original Eq. (c):

$$k\left(\frac{A_r}{A}100\right) = \left(\frac{d}{900}\right)^{\alpha} 13.3\left(\frac{Na_2O}{c}100\right) - 1.17$$

in which α is an unknown constant to be evaluated experimentally.

Thus, the revised equation involves an unverified assumption as to the relationship between x/w_e and d.

We are quite in sympathy with the approach to further experimental work adopted by Professor Poulsen. It amounts to an elaborate statement of a working hypothesis, and reveals the specific kind of information that must be obtained experimentally to test the hypothesis.

Discussion of a paper by Karl V. Steinbrugge and Donald F. Moran:

Performance of Reinforced Concrete and Concrete Masonry in Recent Western United States Earthquakes*

By JOE T. RICHARDSON and AUTHORS

By JOE T. RICHARDSON†

The authors recognize the destruction resulting from the tremendous forces of earthquake shock. They point out several interesting examples of damage to structures, resulting from recent western earthquakes, and offer some solution to the problem of eliminating similar damage in future structures subjected to future earthquakes. The illustrated samples of structural damage may be termed classic examples, as these illustrations are truly representative of typical damage encountered in surveys of the majority of all recent earthquakes.

In the Southern Pacific Railroad Tunnel No. 5, damaged during the earthquake of July 21, 1952, geologic movement on the White Wolf fault sheared the tunnel. No known design could have prevented the damage due to movement of the enormous earth masses involved.

In the walls of Brock's Department Store, the cracks resulting from the Aug. 22, 1952, earthquake aftershock, along the second floor window line at the columns, and similarly at a few first and third floor window-to-column locations, appear to be due to a combination of bending in columns and shear in wall panels. There are indications of possible effects from resonance at points on the southwest corner column and the column to the immediate right, by the cracks indicating a maximum deflection and high bending stress at these points. As mentioned by the authors, damage of this type could possibly have been prevented by eliminating window openings. Additional reinforcement may also be helpful if window openings must be included in a building design.

Damage in the Santa Barbara office building due to the July 21, 1952, earthquake was apparently similar to that in Brock's Department Store. The exterior wall construction of hollow tile panels in concrete frames is apparently very susceptible to damage as a result of bending in columns and failure of panels diagonally in shear. Use of lighter weight and more flexible panels set and well anchored in a cushioned material may be a solution to eliminating such damage.

*ACI Journal, May 1955, *Proc.* V. 51, p. 853. Disc. 51-42 is a part of copyrighted Journal of the American Concrete Institute, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

†Engineer, Structural Behavior of Concrete Dams, Dams Branch, Commissioner's Office, Bureau of Reclamaton, Denver, Colo.

The total damage to the Cummings Valley School in the July 21, 1952, earthquake is a classic example representative of many other such failures in older structures encountering earthquake shock. In this structure lack of adequate reinforcement, poor quality concrete, and improper clean-up of construction joints as mentioned by the authors, all contribute to the lack of resistance to earthquake shock. At the roof line, reinforcement of the corners would have been helpful as a resistance factor.

The damage to the Lockheed plant at Bakersfield shown by the column base was apparently due to bending in the column with a resulting high compression stress at the column edge and failure in shear as indicated by cracks on the surfaces.

Reaction to earthquake of vertical wall sections in the Arvin bank building apparently may have been as the designers intended, with vertical shear cracks developing between the stacks of block that make up the wall.

As the authors of the paper have stated, adequate horizontal bond beams could have contributed to a minimum of cracking.

These examples along with the many more illustrations* of damage resulting from other earthquakes cannot too strongly emphasize the need and care to be exercised in design for earthquake resistive construction, especially in buildings used for public occupancy.

The examination of structures damaged by earthquake in this and other countries reveals that failure in many instances occurs in old buildings of brick, stone, or block construction where the cementing material was a lime or gypsum mortar. With age and mortar deterioration, the structure is not unlike a pile of boards separated by layers of ball-bearings. When subjected to lateral ground movement as in an earthquake, the result is the structure collapses, since the cementing material has no shear resistance. (A good argument for portland cement mortar.) An additional factor contributing to damage and often to loss of life is, that in many instances, particularly in countries of the Far East, a structure is topped with a massive roof of heavy timbered construction and covered with tile or other ceramic material. The center of gravity of the whole topping is thus high above the ground line. Such a roof, supported on walls of brick or block material with a lime cementing material, or supported on wood columns, when subjected to earth movement of the magnitudes recorded in destructive earthquakes, can do little but fall to the ground. Occupants that may have flocked to such structures on occurrence of first ground tremors usually suffered severe casualties. Thus, damage to old or poorly constructed buildings and heavy loss of life are not necessarily an indication of how modern earthquake-designed structures would have withstood the same shocks and these factors are not an accurate index of earthquake intensity.

Only through the classic examples as shown by the authors and a careful

*Steinbrugge, Karl V., and Moran, Donald F., "An Engineering Study of the Southern California Earthquake of July 21, 1952, and Its Aftershocks," *Bulletin of the Seismological Society of America*, V. 44, No. 2B, Apr. 1954 (Second Part).

evaluation of factors related to many known damaged and failed structures, can a comprehensive digest be made that is useful to the engineer faced with the problem of designing for earthquake.

From the many available examples, the engineer preparing designs of a structure in an active earthquake area must recognize that to minimize risk of shock damage, careful selections of type of structure and structural details can contribute much to a well planned design. If possible, structures should be kept as low as practicable, should be founded on solid foundation, should include use of lightweight roofs, should provide for members of the structure to be adequately tied together so the structure will move or vibrate as a unit rather than as separate parts. Of course, this becomes more complicated in massive structures, as dams, powerplants, tall buildings, bridges, and other types of massive structures. The engineer cannot always design from the view of minimum weight and low center of gravity, and is more often required to resort to the accepted methods of providing for the additional earthquake forces imposed on the structure due to inertia of masses, external loading, and effects of resonance.

Closely tied with the visual results of earthquakes and the accepted methods of design are the mass of scientific data rapidly accumulating from the records of acceleration, velocity, and displacement of many of the earthquake shocks of the past 20 years.

These data serve the engineer as a gage, when coupled with the geology and past earthquake history of an area, and through their use a somewhat realistic allowance can be made in design to compensate for the additional forces placed on a structure due to earthquake. At this point the designer may take either of two paths: that of basing compensation for the added forces of earthquake on dynamic factors, or basing compensation on static equivalents of dynamic factors. To go into a discussion of either would involve too lengthy a summarization to include with these comments. However, for either, the beginning is the amount of earth movement, or displacement to be resisted per unit of time, as well as the complex associated wave periods and resulting acceleration. Acceleration is the convenient and accepted index by which compensating factors are introduced into computation for earthquake design, but actual ground displacement and the associated range of earthquake periods is the factor that brings about damage. Wave period and structure period, in conjunction, are the index for determining the effects of resonance of the structure.

All of these factors, when fitted together in the intricate jigsaw puzzle pattern of earthquake design and combined with the earthquake performances of existing structures as presented by the authors, will serve to bridge the existing gap between design and performance and, oftentimes, bring about associated practical economies. Careful planning for the combining of these factors will result in a realistic approach for the engineer who must design a structure that may be subjected to earthquake. He will have the assurance that the structure will perform during an earthquake as had been anticipated.

AUTHORS' CLOSURE

Since the submission of our paper for publication, two more significant earthquakes have occurred in the western United States.

On Dec. 16, 1954, a severe shock occurred approximately 40 miles east of Fallon, Nev. Ground breakage stretched over approximately 50 miles in a north-south direction crossing U. S. Highway 50. Displacements were mainly vertical and reached a maximum of about 18 ft.

The only concrete masonry structures in the area are about 5 miles west of the faulting. These are three small one-story hollow concrete block buildings with wood roofs. One was under construction at the time, in the foundation stage. Mortar is fair. The only damage was "working" at the juncture of the garage to its addition.

Of special interest, was damage to a reinforced concrete reservoir in Sacramento, Calif., 180 miles west of the faulting. This reservoir is 275 x 317 ft with a wood roof on precast concrete columns. Three 3 in. thick reinforced shotcrete baffle walls run in an east-west direction. Surging of the water caused movement of these baffle walls which fractured several of the precast columns.

Eureka, Calif., experienced a strong earthquake on Dec. 21, 1954. Reinforced concrete performed well, and no evidence of structural damage was found in this class. No significant damage was reported to stacks of concrete construction. Two unreinforced brick stacks, which had been damaged in the 1932 shock, were damaged again.

Buildings with unreinforced brick masonry walls and sand-lime mortar suffered, as usual, the heaviest damage. While walls were loosened, no collapses occurred.

Hollow concrete block construction was generally reinforced and had better mortar than the older brick buildings. These features were adequate to prevent all but slight damage.

Discussion of a paper by R. H. Evans and A. S. Parker:

Behavior of Prestressed Concrete Composite Beams*

By PAUL WILLIAM ABELES, K. HAJNAL-KÓNYI, F. WALLEY, and AUTHORS

By PAUL WILLIAM ABELES†

This paper contains valuable information on the cooperation of prestressed and added concrete, particularly with regard to (a) the restraining influence of the prestressed component on the nonprestressed part and (b) the restraining influence of differential shrinkage and creep upon cracking.

This latter question is investigated by the authors who have presented simple formulas which appear to be suitable to analyze the actual stress conditions when all data are known. However, for design purposes it is difficult to select the correct values. It is in most cases impossible for the designer to foretell the conditions of shrinkage and creep in the prestressed beam at the state when the in situ concrete is added, since this depends on the humidity, temperature, and time between prestressing and use (*i.e.*, adding the nonprestressed concrete). Similar considerations also apply to the conditions under which the added concrete hardens onto the prestressed concrete to form a homogeneous composite prestressed member.

In these circumstances, it is somewhat comforting to learn from the test results that the difference between calculated cracking load, ignoring differential shrinkage and creep, and actual cracking load is in most cases small indeed. This can be seen in Table A, in which the values indicate the difference between actual and calculated cracking load, as taken from Table 3. These differences are particularly small for the group C beams No. 6-8.

The other question investigated by the authors is of specific importance to understanding the special advantages of such composite structures. In the paper "Extensibility and Modulus of Rupture of Concrete," mentioned on p. 869, Professor Evans has shown that microscopic cracks were observed in his previous investigations at Leeds University at a state when they were

TABLE A—DIFFERENCES BETWEEN ACTUAL AND CALCULATED CRACKING LOADS

Beam No.	1	2	3	3A	4	4A	5	6	7	8
Difference, percent	+7.5	+13	−5.7	+1.8	−11.4	−5.0	−16.0	+1.1	−1.1	+1.7

*ACI Journal, May 1955, *Proc.* V. 51, p. 861. Disc. 51-43 is a part of copyrighted Journal of the American Concrete Institute, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

†Consulting Engineer, London, England.

still invisible to a search with an ordinary magnifier. In the meantime the writer had an opportunity of ascertaining by strain gage measurements such microscopic (*i.e.*, invisible) cracks, which do not appreciably affect the stiffness of the beam, since a noticeable kink in the load-deflection diagram occurs only when the cracks become visible. This has been shown by the writer in his closure[2] to his ACI JOURNAL paper.[1]

The authors' investigations are based on fine measurements of microscopic cracks, introduced at Leeds University, but not usual in ordinary research. According to the results, stiffness of the composite beam is not appreciably affected, in spite of these microscopic cracks, until a visible crack is noticed across the soffit of the prestressed part, although cracks in the in situ concrete were noticed much earlier. This is in good agreement with the writer's investigations mentioned above. An important conclusion is "that whatever the restraint the extensibility of concrete is unaffected." This means that whenever prestressed and nonprestressed concrete adjoin at the same level of a beam, the nonprestressed concrete will be restrained from visible cracking; but since the limit of its extensibility is reached much earlier than that of the prestressed part, a great number of microscopic cracks will develop which, however, will hardly influence the stiffness of the composite beam until some of these microscopic cracks have developed into visible cracks extending from the outer tensile fiber through the prestressed part to the added concrete.

Advantage can be taken of this phenomenon differently from the authors' advice to place the nonprestressed concrete mainly in the compressive zone. It is stated: "It must be borne in mind, however, that a good composite design does not use in situ concrete at the lower fibers." This is correct if this statement is limited to the *outer* tensile fibers, since obviously designs according to group A and C beams are unsuitable. However, if these "lower fibers" present a nonprestressed concrete which is protected at the outer tensile zone by a thin prestressed skin, advantage can be taken of the phenomenon of restraint. The writer fully agrees with the authors' views as

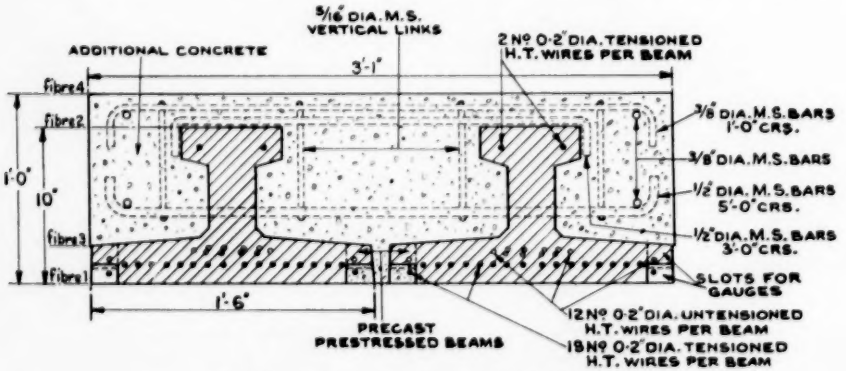


Fig. A—Cross section of composite bridge slab

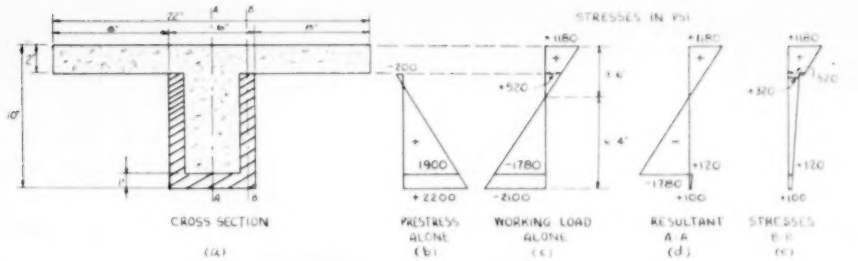


Fig. B—Proposed composite beam

long as they are limited to the three shapes of Fig. 1. However, a shape shown in Fig. A has not been considered in detail by the authors, although some advantage can be taken of the tensile resistance of the added concrete restrained by the prestressed concrete underneath. Sufficient roughening of the prestressed concrete surface has proved to insure satisfactory bond with the in situ concrete, though the writer knows of cases where insufficient roughening resulted in premature bond failure. Fatigue tests on the composite slabs of Fig. A have shown that the cooperation of the prestressed and added concrete is not impaired by millions of repetitions of loading during which cracks open and close. The possibility that "the lower in situ concrete necessary for bending will probably be cracked at cracking load and will have lost its bending resistance," which the authors mention, does not apply in this case and is apparently based on the behavior of beams of the A and C group in which the in situ concrete is only stuck sidewards to the prestressed concrete and not enclosed as that in Fig. A.

A further development of this idea is a solution in which the in situ concrete is protected and restrained by a prestressed concrete skin at the tensile zone, which skin preferably remains in compression under working load, as suggested by the writer[3] and shown in Fig. B. In this case use can be made of an increased tensile resistance of the added concrete to an extent never foreseen as possible. Before this is discussed further, reference may be made to the authors' findings that "an in situ concrete crack induces a stress concentration and is liable to cause early cracking of the prestressed concrete near the joint." This will certainly be distinct for beams of the A and C group and similar constructions in which the nonprestressed concrete is at the outer tensile fiber of the composite beam, but will be of much lesser influence with beams in which the entire outer tensile skin is prestressed, and is of little influence if this skin is as thick as that in Fig. A. Assuming that a beam according to Fig. B(a) is shored before the in situ concrete is inserted into the prestressed channel, that the effective prestress at the bottom fiber is 2200 psi in compression and that at the top 200 psi in tension [see Fig. B(b)], that the depth of the skin is 1/10 of the entire depth, and that the working load stresses in a straight-line stress distribution are 1180 psi in compression and 2100 psi in tension, as indicated in Fig. B(c); then the maximum result-

ant concrete tensile stress in the added concrete would be 1780 psi and ,nevertheless, cracking in the prestressed skin would be avoided. From the resultant stress distributions at sections A-A [Fig. B(d)] and B-B [Fig. B(e)] it is seen that there are only compressive stresses in the skin and thus an early cracking due to stress concentration should be avoided. These simplified stress distributions are based on the assumption that there is no appreciable difference in the modulus of elasticity between prestressed and added concrete. The prestressed skin may be monolithic or composed of prefabricated blocks with post-tensioned steel members embedded in the added concrete.

The possibility of employing restrained, nonprestressed concrete up to a working load tensile stress of, say, 1780 psi and still insuring freedom from visible cracks seems to be an attractive solution, particularly when considering that these microscopic cracks which would be completely harmless if they were at the outer surface are in this case inside the beam.

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By HAJNAL-KÓNYI*

Being responsible for design of the concrete construction system known as the "Myko" floor, I would like to make a few comments.

This type of floor and roof has been in use in Great Britain since 1940, when the shortage of timber called for introduction of a lightweight concrete floor. Fig. C shows the typical cross section of a Myko floor in its original form. The foamed slag concrete casing of the beams is nail holding and allows the fixing of a ceiling to form a horizontal soffit without the need of any timber, and reduces the weight of precast materials to be handled. With the development of factory made prestressed concrete and with timber being available, the design of the floor was adapted to the application of prestressed units (Fig. D). The floor as indicated in Fig. C has been used up to spans of 18 ft but from 16 ft upwards it is preferable to use the type shown in Fig. D. The maximum span reached so far is 28 ft, but an extension of this limit is anticipated.

It may be seen from a comparison of Fig. D with Fig. 1 that the composite beams tested by the authors were not truly representative of a Myko floor in which the volume of in situ concrete in relation to that of the precast beam is much greater. This discrepancy may account for the shrinkage cracks observed by the authors which do not occur in practical appications.

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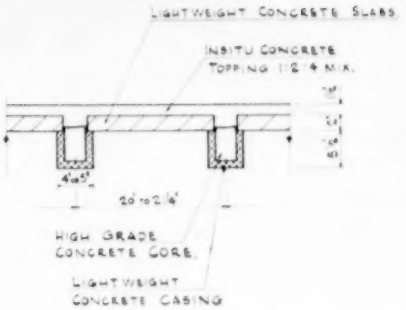

Fig. C—Typical cross section of a Myko floor reinforced with mild steel or cold worked steel

Beams tested at the Imperial College of Science and Technology, London University, in 1951–52[1] were much nearer to practical conditions. Nevertheless, the authors' results and conclusions are valuable. They have found the performance of the beams in group B (which they call Myko) superior to that of the beams in groups A and C.

The authors are right in stating that Fig. 8 and 9 do not show an ideal design, but the distribution of stresses in a prestressed Myko floor is rather different since the in situ concrete is almost entirely in the compression zone, in accordance with the authors' recommendation. The floor is so designed that the maximum fiber stress under working loads does not exceed 1000 psi. The prestressed units have a cube strength of 6000 to 7000 psi at 28 days, while the specified cube strength of the in situ topping is only 3000 psi at 28 days. The top surface of the prestressed units is "keyed" over the whole length and where the horizontal shear exceeds 50 psi under working loads stirrups protruding from the precast units are provided which are capable

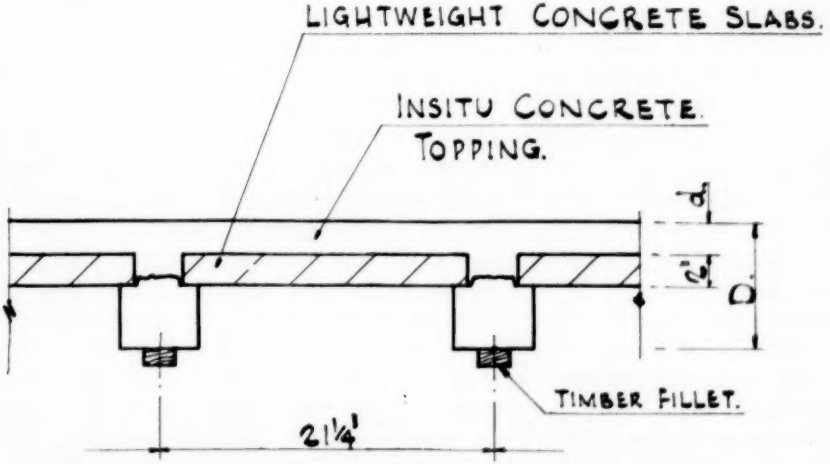

Fig. D—Typical cross section of a Myko floor with prestressed concrete beams
Note: Depending on span and loading, D varies from 8 to 15 in., and d varies from 2 to 3 in.

of transmitting the full shear force. In this way, a 100 percent efficiency of bond between precast and in situ concrete is insured so that the full strength of the prestressed wires is utilized if the floor is tested to failure.[1] It should be noted that in the authors' tests shear failure only occurred when the beams were tested on a 4-ft span, *i.e.*, when the shear span was reduced to 2 ft. The beams are, of course, not designed for this type of loading which never occurs in the floors of a span of 16 ft or more.

The "time between prestressed cast and in situ cast" (see Table 2) cannot be fully controlled because of possible delays on the site, but the aim is not to exceed 4 to 6 weeks. Consequently, the conditions of the completed structure are somewhere between cases 2 and 4 and the differential shrinkage coefficient is approximately zero.

The paper supplies justification for the method of design which, based on similar consideration as given by the authors, ignores the effect of differential shrinkage with no adverse effect on the cracking load.

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By F. WALLEY*

The paper is an important contribution to the understanding of a method of construction which is widely used. It is, indeed, of almost universal application in floor or bridge deck construction, whether in steel, reinforced, or prestressed concrete. What is said in this paper is applicable to all forms of construction. In fact, in normal steel-framed construction, the stresses induced from the shrinkage of in situ concrete floors can be almost as significant as the applied loads. It is, thus, important that investigation of the stresses induced should be undertaken. Failure tests, pure and simple, show nothing, since the failing load would not, in general, be altered, but the working load conditions can be altered as the authors show.

The test specimens chosen, particularly groups A and C, are not perhaps ideal. The usual type of construction employs a T-beam with a prestressed stem and in situ deck, and in this system the shrinkage effect is probably greatest because of the large eccentricity at which the shrinkage force acts.

The method of calculating the effects of differential shrinkage do not appear to take into account the way it may be modified by creep. This generally reduces the effect, in some cases (theoretically) by half. A method based on Mörsch's work is outlined in "Prestressed Concrete Design and Construction."†

The authors emphasize the need of good bond, and this, in many cases, has been achieved by using short length upstands which have in construction and under test proved efficient. It is interesting to record, however, that

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†Walley, F., "Prestressed Concrete Design and Construction," Her Majesty's Stationery Office, London, England, 1953.

flat paving slabs simply resting on the top of the Ministry of Works' joist gave a good measure of composite action under a test.

AUTHORS' CLOSURE

The authors are grateful to all who have contributed to the written discussion. Mr. Walley states that the effect of concrete shrinkage may have considerable effect upon the stresses in certain types of construction and he also makes the point that the authors' method of calculating the effect of differential shrinkage in composite sections does not appear to take into account the effect of creep.

But a distinction must be made here between the creep of the prestressed member under prestress, and that creep due to the stresses caused by differential shrinkage which will tend to relieve those stresses. It was intended in the first place, that in assessing the "differential shrinkage coefficient" the whole predicted movement of the prestressed concrete due to its own shrinkage and creep under prestress should be allowed for. The practical value of the method depends upon an accurate assessment of this movement to be set against the shrinkage of the in situ concrete. It is considered that good results will be obtained by taking an average creep over the prestressed member, even though in certain composite sections the line of bond may be mainly in the region where the prestress and the resulting creep are low. The average creep assessed for the centroid will tend to transmit its effect throughout the combined section once the member becomes composite.

With regard to that creep caused by the stress, or change of stress, due to differential shrinkage, creep in the elastic range is approximately proportional to stress and may be allowed for by reducing the values of Young's modulus used in computing the stresses E_1C_1, etc. Mr. Walley suggests that this may reduce the effect theoretically by as much as 50 percent. But if secant moduli at working stresses are used, these are already lower than the initial tangent moduli and in the authors' opinion it is doubtful whether prolonged creep at the low stress changes involved would justify much further reduction.

Dr. Abeles has worked out the percentage alteration in cracking loads in the test beams thought to be due to differential shrinkage, and observes that they are low, but as Mr. Walley points out, in a T-beam type of construction these may be considerably higher and it would seem advisable to make at least a qualitative prediction in design even if the data were not sufficient for an accurate quantitative one.

Dr. Abeles has made valuable and interesting suggestions for utilizing the type of restraint of the added concrete which was observed in the investigation. His stress diagrams give a clear picture of the way in which he proposes that this should be done.

He points out the attractive nature of the possibility of employing non-prestressed concrete up to a tensile stress of 1780 psi but in the authors' opinion if cracks were present at the stress, however small they were, some of

the tensile zone would have to be neglected. The question would appear to be "how much?"

Although the investigation was confined to added concrete placed on the outside of prestressed members, the evidence of this and other investigations is that the greater the area of bond between the concrete which has reached its limit of extensibility and some uncracked surface, the greater is the number of cracks and the smaller their size. This points to the possibility, as Dr. Abeles suggests, that if the whole of the tensile zone of added concrete was enclosed by the prestressed member, the restraint would be such that the cracks would not extend in the normal way due to the resilience of the uncracked portion. Thus a modification of the classical "excluding tension" theory to include that added concrete in the tensile zone up to the tensile limit would appear to have some justification. Also, due to the degree of restraint operating, this tensile limit should be determined from a modulus of rupture test specimen rather than from a tensile test.

Referring to the figures given by Dr. Abeles in the stress diagrams, the maximum tensile stress in the added concrete was 1780 psi. The modulus of rupture stress might be of the order of 700 psi and thus rather less than half the added concrete below the neutral axis of the combined section can be considered to retain its stiffness at working load. There would, of course, be a slight rise in the neutral axis and for design purposes a balance would have to be achieved. Whether the restraint would be such that a higher modulus of rupture stress could be used is not predictable from the present investigation but there was no evidence whatsoever of this in concrete placed outside the prestressed members.

Dr. Abeles mentions cases where insufficient roughening resulted in premature bond failure and, as he points out, it would be necessary to be sure of successful bond if the above theories were to hold good for repeated variations of live load. The repeated load experiments made by Dr. Abeles show that the stiffness would not be impaired by millions of repetitions of loading if steps were taken to insure good bond.

With regard to the use of prefabricated block for the prestressed channel section, these might be a weakness if the joint of the block was such as to impair the continuity of the high quality bonded concrete surface.

Dr. Hajnal-Kónyi has made it clear that the "Myko" beams as tested were not truly representative of the function for which they were designed. The scope of the investigation was not sufficient to include proper floor units, but the beams which were made served to illustrate the way in which the added concrete could successfully be confined to the compressive zone.

The authors agree with Dr. Hajnal-Kónyi in stressing the importance in this type of construction of making adequate provision to carry the horizontal shear between the two concretes. It is interesting to note that in the practical construction of the Myko floor, the dates of casting the prestressed element and adding the in situ concrete were arranged so as to neutralize differential shrinkage and to give a coefficient of approximately zero.

Discussion of a paper by J. W. Kelly and Milos Polivka:

Ball Test for Field Control of Concrete Consistency*

By E. L. HOWARD, THOMAS B. KENNEDY, BRYANT MATHER and C. H. WILLETTS, PHILLIP L. MELVILLE, and AUTHORS.

By E. L. HOWARD†

The Kelly Ball was loaned to us by Professor Kelly in 1948 to try out on some ready-mixed concrete jobs. Our satisfaction can be measured by this fact: after one week's use of the borrowed ball-penetration apparatus we had our own shop making them. These have become standard equipment for our service and inspection departments.

We agreed to make some slump–Kelly Ball comparisons for a paper Professor Kelly was preparing. This project was never properly completed because our technicians "lost" their slump cones after experience with the Kelly Ball. At this writing our organization (12 technicians) has only one slump cone in use, and that only when specifically requested by a customer. Since 1949 all control tests for workability have been made using the ball-penetration apparatus.

The fellow on the job finds the slump test too laborious to take himself, too time consuming when taken by inspectors. The foreman, most likely, won't believe the slump cone results anyway. Concrete consistency on most jobs is controlled by the "eyeball" test. This test can be made instantly but all errors favor the wet side at the expense of specifications. The Kelly

TABLE A—KELLY BALL–SLUMP COMPARATIVE TESTS ON JOB-SAMPLED CONCRETE

Job	Reinforced concrete walls	Sidewalk and curbs	Concrete piling	City streets	Reinforced concrete beams and slabs
Cement, sacks per cu yd	6.2	5.0	6.12	6	6.5
Maximum aggregate, in.	¾	1½	¾	2½	¾
Kelly Ball					
Average penetration, in.	2.5	2.75	2.5	1	2
Minimum–maximum penetration, in.	2–3.5	2.5–3	—	0.75–1.75	1–2.5
No. of samples	10	4	1	5	12
Slump					
Average, in.	5¼	5½	6	2	4
Minimum–maximum, in.	3½–7	5–6	—	1–3	2½–5
No. of samples	10	4	1	5	12

*ACI JOURNAL, May 1955, *Proc.* V. 51, p. 881. Disc. 51-44 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

†Testing Engineer, Pacific Coast Aggregates, Inc., San Francisco, Calif.

Ball test, we found, was fast enough for the concrete foreman and satisfactorily accurate for the engineer.

On some jobs comparative tests were made using the slump cone and the Kelly Ball. Test results from typical jobs are shown in Table A. Most concrete men think of workability in terms of slump. A factor for translating the inches penetration to slump is found to be approximately 2. A penetration of 2.5, for example, indicates a 5-in. slump.

The variety of jobs shown in Table A is indicative of the wide range of concrete work that can be tested and controlled with the Kelly Ball.

By THOMAS B. KENNEDY, BRYANT MATHER, and C. H. WILLETTS*

The authors mention that the ball-penetration test has been adopted by the Concrete Division of the Waterways Experiment Station of the Corps of Engineers, U. S. Army. "Method of Test for Ball Penetration of Freshly Mixed Concrete" (Designation: CRD-C 46-53) was distributed in December, 1953, as a supplement to the *Handbook for Concrete and Cement* for use throughout the Corps of Engineers. The "Tentative Method of Test for Ball Penetration in Fresh Portland Cement Concrete," prepared by Subcommittee III-c, I. L. Tyler, chairman, was approved by ASTM Committee C-9 and included in its 1955 annual report. It was accepted by ASTM at the annual meeting in June, 1955, for publication as ASTM Designation: C 360-55T in the 1955 *Book of ASTM Standards*. Designation: C 360-55T includes some modifications of apparatus and procedure from those the authors presented in the appendix to the paper. It requires that each foot of the stirrup have a minimum bearing area of 9 sq in. It provides for taking readings to the nearest ¼ in. and reporting the average of three or more readings which agree within 1 in.

The authors refer to tests by the Concrete Division, Waterways Experiment Station, in which an average ratio of slump to penetration of 1.79 was found. Additional tests on a variety of concretes have been made, with crushed stone coarse and fine aggregate. Some of the results are summarized below:

Coarse aggregate size, in.		¾	3	6
No. of tests		397	60	295
Penetration, in.	Maximum	3¼	¾	1¼
	Minimum	¾	0	0
	Average	1¾	¼	½
Slump, in.	Maximum	3½	4*	5*
	Minimum	2	¼*	½*
	Average	2¾	2¼*	2*
Ratio: slump/penetration		1.59	9	4
Correlation coefficient		0.80	0.86	0.076

*Slump test made on concrete wet screened over a 1½-in. sieve, penetration test made before sieving.

*Civil Engineers (Concrete Research), Concrete Division, Waterways Experiment Station, Corps of Engineers, U. S. Army, Jackson, Miss.

The ratio of slump to penetration based on nearly 400 tests of ¾-in. aggregate concrete is now 1.59 rather than 1.79 as previously reported. These two values bracket the 1.66 ratio that was found in the National Ready Mixed Concrete Assn. tests. The results of the tests on concrete with 3-in. and 6-in. aggregate indicate that there is little ball penetration in such concrete even when the slump of wet-screened batches is as much as 4 or 5 in.

We believe that the ball test is a good tool for checking production consistency of concrete. It can be performed quickly and easily—so quickly and so easily that perhaps its potentialities have not always been appreciated. It does not give spectacular results. With normal good concrete, the ball does not sink out of sight. Since the values obtained are small numbers, generally about half the magnitude of slump values, the operator may get the impression that little is happening in the test. Perhaps if the ball moved more during the test, so that about 4 in. of movement took place in "4-in. slump" concrete, the test would be more satisfying to operators conditioned to slump tests. If increased movement were obtained by materially increasing the weight of the ball, the disadvantages of the change would probably outweigh the advantages.

By PHILLIP L. MELVILLE*

The large number of tests devised to evaluate the workability of concrete is an indication of the importance of the problem and of the difficulty in finding a satisfactory solution. The slump test has found some favor (probably mostly by default) albeit any claim that it allows "measurement of the workability" is open to serious question as concrete technologists know only too well. The development of a field test to control concrete consistency is to the credit of Professor Kelly and his associates. Kelly and Polivka have well stated their case in favor of the ball test and the writer is in general agreement with them.

Although the Virginia Department of Highways specifies the use of the slump test to control consistency in its latest revision of *Road and Bridge Specifications* (April, 1954),† the Division of Tests of the department has listed, and described, the penetration test in its 1955 *Manual of Instructions.*‡ The ball test, barring unforeseen unfavorable developments, seems to be nearing acceptance in the state. To investigate the advantages of accepting the new test, the Virginia Council of Highway Investigation and Research (a cooperative agency sponsored jointly by the Virginia Department of Highways and the University of Virginia) undertook a field and laboratory study early in 1954.§

Three groups of tests were performed in the laboratory on different concrete mixes. It was found that for equal mixes the penetration increased in relation

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†Virginia Department of Highways, *Road and Bridge Specifications*, 1954.

‡Virginia Department of Highways, Division of Tests, *Manual of Instructions*, 1955.

§Melville, P. L., "Progress Report 1—Control of Concrete Consistency," Virginia Council of Highway Investigation and Research, 1954.

to the slump with increased entrained air and with decreased fineness modulus of the over-all gradation.

At that time, it was attempted to establish slump equivalents to fit existing specifications. This is now viewed as impractical and it is believed that the greatest value of the ball test may be in controlling uniformity of consistency. After a concrete mix has been adjusted in trial batches from the initial design to meet job requirements (which may or may not include a specified slump), a standard penetration could be recorded. Throughout the remainder of the job, for that given mix, the penetration would remain the same (with a tolerance usually set at ± ¼ in. penetration). This would be particularly helpful in enabling the inspector to reject improper batches as they come from the mixer and before they have been used.

The department furnished several field inspectors on various projects with proper equipment for the ball test. The reaction from the field was not as encouraging as expected. The objections raised, such as weight and cleaning, cannot be considered valid and it is thought that the cautious feeling may be more a reluctance to change and may also reflect the concern of some field men toward a new, fast, and rigidly enforceable test.

More balls are being tried in the field and it is possible that others that have been made corrosion-proof (either by plating or by the use of selected metals) will be issued. It is hoped that their efficient extensive and intensive use will make for better concrete through easier uniformity consistency control.

AUTHORS' CLOSURE

The preceding discussions are gratifying and confirm the favorable reception which the ball test has been accorded in practice. Mr. Howard's experience of several years is with the variety of concretes found in structural work and ready-mixed concrete in general; Mr. Melville's experience is in highway construction, for which the test is perhaps most suitable; and Messrs. Kennedy, Willetts, and Mather have investigated the test on mass concrete, using the slump of wet-screened concrete for comparison.

The data presented in the discussions confirm the authors' conclusion that use of a fixed general ratio between slump and penetration is impracticable. However, under given steady-state conditions, a ratio might be established by trial and used for the particular purpose. Neither the slump test nor the ball test are absolute measures of a fundamental quantity; rather they are means of controlling uniformly any concrete consistency which has been established by those in authority from direct observation or other considerations. It is submitted that the ball test accomplishes this end more quickly, with less likelihood of personal error, and wherever desired on undisturbed samples.

With regard to the large variation in slump-penetration ratio found for mass concrete in the Corps of Engineers' tests, it is believed that one factor was the low values of average penetration (¼ and ½ in.) which were close to the

error of test. In tests by Professor Polivka, reported in the January 1950 *ASTM Bulletin*, the ratio of slump of wet-screened concrete to penetration of stiff "mass" concrete (2½-in. aggregate) was 1.65, the average slump of the wet-screened concrete being about 1¼ in. and the average penetration in the full mass mix about ¾ in.

The test data reported in discussion by Mr. Howard disclose slump-penetration ratios for the five classes of concrete of 2.1, 2.0, 2.4, 2.0, and 2.0, respectively.

Several modifications of the test apparatus and procedure were made by Subcommittee III-c of ASTM Committee C-9 when the test was adopted as a tentative standard (C 360-55T), as follows:

(a) *Larger stirrup feet*—The minimum bearing area of each stirrup foot is specified to be 9 sq in. Existing sets can be modified to meet this requirement. Although not stated, the ASTM drawing suggests that a semicircular plate of 5-in. diameter be used.

(b) *Size of sample*—C 360-55T requires a minimum depth of concrete equal to at least 3 times the maximum size of aggregate and at least 8 in.; it also requires a 9-in. minimum radius of sample. Thus the minimum ASTM sample requires normally about 1.2 cu ft (180 lb) of concrete whereas our recommendation stated in the paper was about 0.4 cu ft (60 lb). The larger sample is undoubtedly desirable, as the ball needs plenty of room to avoid particle and boundary interference, but in the authors' opinion it is unduly conservative. Perhaps some intermediate value would suffice.

(c) *Precision of reading*—C 360-55T specifies graduation, reading, and reporting to the nearest ¼ in., which is roughly comparable to slump readings to the nearest ½ in.

Other suggested modifications, which are not inconsistent with C 360-55T and which the authors believe to be desirable, are the use of corrosion-resistant metal and of a larger (easier reading) graduated rod; these are already available from some manufacturers.

For special uses such as unusually stiff concrete, mass concrete, or studies of rate of stiffening after placement of pavement, a heavier ball might be used even though the test would not be standard. Slotted disks 6 in. in diameter and weighing 10 lb could be laid on the 30-lb ball as desired, with corresponding modification of the stirrup and riser rod for clearance. Similarly, a lighter (20-lb) 6-in. ball has been successfully used for lightweight concrete.

In conclusion, the authors hope that the principal objective of the ball-penetration test will continue to be attained—more frequent actual testing and closer control of consistency. It is well known that variable and excessively wet consistencies are often major contributors to poor concrete.

Discussion of a paper by Earl I. Brown, II:

Strength of Reinforced Concrete T-Beams Under Combined Direct Shear and Torsion*

By PHIL M. FERGUSON†

Some aspects of this paper deserve further discussion. For example, from the design viewpoint, what are the significant facts indicated by this investigation? One certainly is that the calculation of torsional stresses by elastic theory and their addition to the usual shearing stresses is of no design value. Ultimate load shearing stresses calculated on this basis averaged 240 percent of those causing failure under zero eccentricity of load. It is thus apparent that elastically calculated shearing stresses due to torsion are much too high to be realistic or meaningful. This may be due to at least two favorable factors that are present. One is that the compressive stresses due to bending moment greatly reduce the diagonal tension which might otherwise be developed in the compression or flange area as a result of large unit shearing stresses. The second is that, while torsional shears increase the shear on one side of a beam, they reduce the shears on the opposite face. It is evident that these beams were able to mobilize these, and possibly other reserve strengths, sufficiently to make the maximum elastic shearing stress only 42 percent as dangerous as in centrally loaded beams.

Dr. Brown's study indicates that torsional stresses are significantly smaller when analyzed by plastic theory. Plastic stresses range from something like 60 percent as high for rectangular beams to a little over 50 percent as high for T-beams of the size studied (Table 2). This is in the right direction, as evidenced by the tests, but this theory also is inadequate. Applied to initial cracking loads, it gives stresses 15 percent higher than the calculated failure stresses for centrally loaded beams. Since failure loads averaged 50 percent above cracking loads, the ultimate stresses, if calculated by this procedure, would average 72 percent above those for failure loads on centrally loaded beams. Thus the use of the plastic theory, although conservative, is likewise not a suitable procedure for an economical design. There are still strength resources available that have not been separated and evaluated.

Should these strengths beyond the first cracking load be ignored in design? Dr. Brown makes the point that neither plastic nor elastic analysis applies after cracks form. He might have gone a step further and have noted that the ordinary relationship for direct shear, $u = V/bjd$, likewise does not actually apply to a cracked section, since most of the shear must be crowded

*ACI Journal, May 1955, *Proc.* V. 51, p. 889. Disc. 51-45 is a part of copyrighted Journal of the American Concrete Institute, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.

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into the compression area above the crack. Nevertheless, our allowable unit shear stresses are based on ultimate loads. An inspection of the deflection curves of Fig. 5 and the rotation curves of Fig. 6 indicates that the behavior of the specimens after cracking is not radically different from that before cracking. The deflection and rotation rates increase, but not more significantly than does the deflection of the centrally loaded beams. The ultimate strengths in these tests seem to be as valid as other ultimate strengths commonly used in appraising the factor of safety.

In the absence of a satisfactory theory for ultimate strength, design for torsion is necessarily somewhat handicapped and crude. The complications due to torsion have generally been ignored in reinforced concrete design and it is notable that harmful cracking has not occurred often, if at all. Table 1 indicates that an eccentricity of load equal to 0.75 to 1.25 times the web width reduces the load capacity only 15 percent for the T-beams, 20 percent for the L-beam, and 40 percent for the rectangular beam. In the case of the T-beams it requires an eccentricity of over 3.1 times the web thickness to reduce the load capacity to 40 percent. The ratio of the shear V' (for $e = 0$) to the maximum shear in Table 1 gives data the designer can use with some assurance in design.

The writer would be slightly more conservative in the case of torques that were statically determined than in the case of those due to continuity alone. In the latter case it is noted that the angular rotation of a well designed slab, freely supported, will not usually be as large as the rotation of these specimens at initial diagonal cracking. Hence it appears that torques produced by such slabs will not be large enough to produce diagonal cracking at working loads, even when the beams are designed without regard to torque. This does not necessarily hold for overload conditions.

Some questions might be raised as to whether marginal beams should be designed as L-beams or rectangular beams insofar as torsion is concerned. An exact analysis might logically make the separation into rectangular beam and slab. However, both slab and rectangular beam are available to carry torsion and the use of the L-section is probably a convenient method for general use, especially when approximate values of the torque are utilized.

Dr. Brown's studies have answered somewhat the question as to why more beams have not failed as a result of torsional shears. Such shears, when combined with moment, are simply not as serious as elastic calculation methods had indicated. Research is still needed to define the mechanism by which an unexplained 72 percent of "extra" strength is realized, even when calculated on the plastic basis. There are some cases where torsional stresses at least appear to be of considerable importance and still more information would be valuable.

Discussion of a paper by G. M. Bruere:

Air Entrainment in Cement and Silica Pastes*

By S. B. HELMS, THOMAS B. KENNEDY, and AUTHOR

By S. B. HELMS†

There is a special need for the type of physical performance data of surface-active materials in cement pastes which the author has reported, particularly when the various agents are clearly defined and classified according to their chemical nature.

The behavior of air-entraining chemical substances in pastes is often different than in mortars and concretes and higher concentrations are needed to develop foams in the absence of aggregate particles in the sand size range. At higher concentrations the effect on strength of the admixed surface-active agent may become an important consideration. The author should consider supplementing his data on efficiency of the foaming agents with information on the effect of these agents on the strength of the hardened foamed pastes; this information would probably be accepted as worth while even if results for certain agents were not included.

This writer can recall working with a sample of Igepon-T more than 10 years ago and this material was efficient as a foamer for cement paste but the small concentrations involved reduced the early strength to a very low value.

Many readers of Mr. Bruere's article will think in terms of the application of his results to the entrainment of air in concrete by some of these untried materials. It is desired to present some interesting test results which show that improved frost resistance should not be taken for granted simply because an agent appears to incorporate the proper amount of air. The fact that two nonionic agents were studied by the author prompts us to give a brief account of some freezing and thawing tests of concrete containing a similar substance. Our short study was made to confirm the results of other investigations[1,2] by making tests under different conditions of exposure using our laboratory stock supplies of cement and aggregate and the same nonionic agent as air-entraining admixture. The agent interground with the cement was one of the materials which meet ASTM specifications. Characteristics of our test concretes are shown in Table A.

*ACI Journal, May 1955, *Proc.* V. 51, p. 905. Disc. 51-46 is a part of copyrighted Journal of the American Concrete Institute, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.
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TABLE A—DATA ON ¾-IN. MAXIMUM GRAVEL CONCRETE MIXES

Mix No.	Cement content, sacks per cu yd	Admixture, fluid oz per sack	Air content, percent	Measured slump, in.	*W/C* ratio, gal. per sack	Compressive strength, psi	
						7 days	28 days
20	5.20 Type I	none	1.7	5	7.5	2310	4370
21	5.15 Type IA	none	6.2	6	6.6	2050	3650
22	5.17 Type I	0.39	6.3	6	6.5	2230	4070

The freezing and thawing test specimens were 3 x 3 x 15-in. concrete bars containing gage points and compressive strength specimens were 3 x 6-in. cylinders; all specimens were made in steel molds. Test bars were cured 14 days moist and 14 days in laboratory air and were resaturated by 24-hr soaking prior to the first freezing. Specimens were subjected to daily cycles of freezing in 0 F air and thawing in 40 F water.

Progress of the exposure was followed by length change measurements and tests for dynamic *E* at selected intervals. Specimens were raised to 70 F for a brief period when readings were taken. The exposure was terminated after 200 cycles of freezing and thawing and the condition of the troweled top surfaces of the specimens is shown in Fig. A.

The measurements showed a drop in dynamic *E* of two of the three non-air-entrained concrete specimens from mix No. 20 between 175 and 200

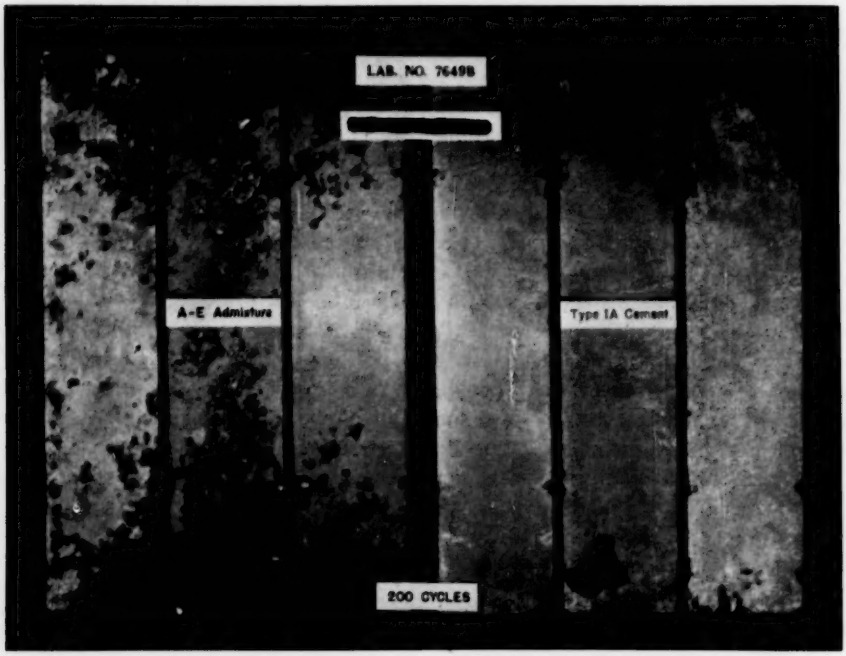

Fig. A—Troweled top surfaces of 3 x 3 x 15-in. bars after exposure

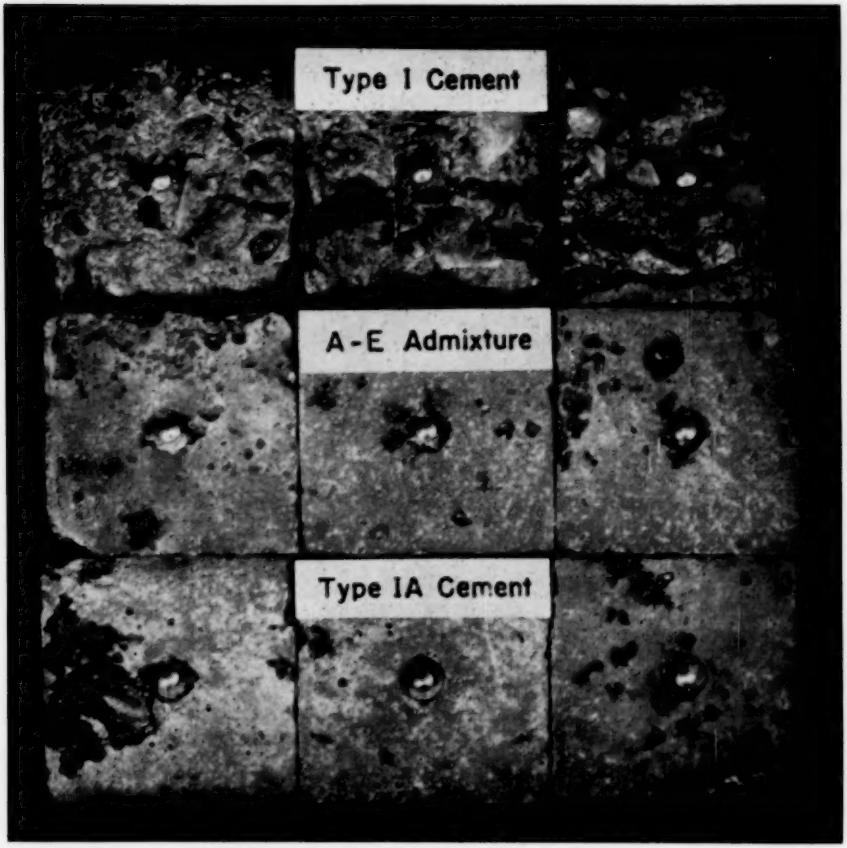

Fig. B—Ends of 3 x 3 x 15-in. bars after exposure

cycles which was accompanied by an expansion of more than 0.1 percent. Specimens from mixes No. 21 and 22 which were air-entrained concretes did not show any significant changes in either sonic E value or expansion during the 200 cycles of exposure.

The important result was the finding of excessive surface scaling on the specimens which contained the particular nonionic type of air-entraining admixture.

In Fig. A the notched effect at edges is due to mechanical contact with specimen racks and should be disregarded; the deep seated surface defects found on several specimens are to be associated with inferior particles of coarse aggregate.

In Fig. B, illustrating the mixes of Table A, there is no observable advantage for mix No. 21 Type IA specimens compared to mix No. 22 admixture specimens but the superiority of the air-entrained concretes is evident. However,

the Delaware River aggregates used showed excellent performance in the case of the plain mix when compared to some other aggregates.

REFERENCES

1. "Tests of Air-Entraining Admixtures," *Technical Memorandum* No. 6-327, Waterways Experiment Station, Corps of Engineers, U. S. Army, Jackson, Miss., Aug. 1951.

2. Scripture, E. W., Jr., Benedict, S. W., and Litwinowicz, F. J., "Air Entrainment and Resistance to Freezing and Thawing," ACI JOURNAL, Dec. 1951, *Proc.* V. 48, pp. 297-308.

By THOMAS B. KENNEDY*

Much fundamental work needs to be done in the surface chemistry of air entrainment. Mr. Bruere is to be congratulated on the work he reports; much more needs to be done. Mr. Bruere's findings appear to explain why the air-entraining capacity of a neutralized resin solution remains unimpaired even though a precipitate forms in the presence of calcium chloride.

The presence of pits (air voids) on structural concrete surfaces is objectionable in many cases, particularly if it is desired to paint the surfaces without filling the pits. I wonder if Mr. Bruere doesn't hold the key to one of the causes of excessive pitting.

AUTHOR'S CLOSURE

The comments by Mr. Kennedy and Mr. Helms raise interesting points. The work on air entrainment in pastes was intended to show the mechanism of certain types of surface-active agents in entraining air and to serve as a basis for further studies on the action of surface-active agents in concrete. Mr. Helms correctly warns that air-entraining agents should not be used in practice until it is known how they affect the strength and durability of concrete. Work is in progress in this laboratory to study the air-entraining mechanisms in mortars and concretes. In addition, studies of strength and durability of air-entrained concretes and effects of type of agent and formwork on surface pitting are receiving consideration.

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Discussion of a paper by T. Y. Lin:

Strength of Continuous Prestressed Concrete Beams Under Static and Repeated Loads*

By ERDEM ARDAMAN†

Professor Lin should be congratulated for his findings in his static and fatigue testing of prestressed continuous beams. The experimental data obtained are in good correlation with classical elastic theory.

As Professor Lin indicated, tensile strains in concrete vary greatly. This is largely due to the nature of concrete deviating from homogeneous and isotropic conditions under tension. On the other hand, errors in strain measurement are also anticipated on the surface of the concrete in compression. This fact can be attributed to the different moduli of elasticity of the aggregate and the mortar. In this light the maximum error occurs on small gages placed directly over the aggregate.

In similar static and fatigue testing of simply supported prestressed concrete beams at the University of Florida, we were also faced with the problem of the size of electrical resistance type strain gages to be used on these beams.

In striving to establish a correlation between the effect of gage length and size of aggregate on the accuracy of strain measurement, a series of tests were run on standard concrete cylinders 6 in. in diameter and 12 in. high. Various sizes of electrical resistance strain gages and extensometers were placed on the specimens in close proximity to measure strains along small as well as large lines of stress. Larger gages were used to check the total strain as measured by the summation of the strains obtained from smaller gages.

Different concrete cylinders were cast with varying maximum size of aggregate. In such an analysis it was possible, as far as the extent of the study was concerned, to establish a relationship between the size of the aggregate and the nominal size of the resistance strain gage to be used within an anticipated maximum error.

As shown in Fig. A, when the nominal length of the strain gage approaches the size of the aggregate, the maximum error could be as high as 40 percent. This maximum error, however, can be decreased by increasing the ratio of the gage length to aggregate size.

*ACI JOURNAL, June 1955, *Proc.* V. 51, p. 1037. Disc. 51-53 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 27, No. 4, Dec. 1955, Part 2, *Proceedings* V. 51.
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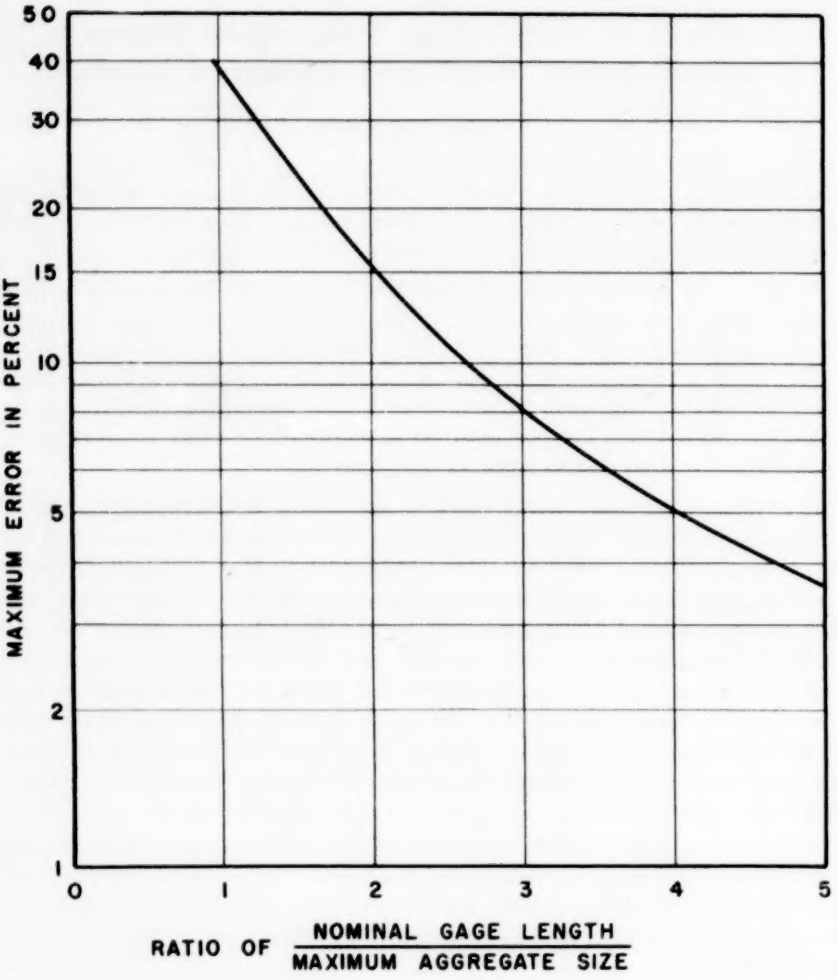

Fig. A—Relationship between maximum error and ratio of gage length to aggregate size

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